NEW PRINCIPLES OF GUNNERY:

CONTAINING,

The Determination of the FORCE of GUN-POWDER,

AND

An Inveſtigation of the Difference in the RESISTING POWER of the AIR to *Swift* and *Slow Motions*.

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PREFACE.

ABOUT a Twelvemonth ſince, I had ſome Intentions of exhibiting a public Courſe of Fortification and Gunnery: And tho' for Reaſons not neceſſary to be here mentioned, I afterwards deſiſted from that Deſign; yet, as I had proceeded ſo far as to diſtribute ſome Manuſcript Copies of the Particulars, of which I propos'd it to conſiſt, I have thereby been in ſome Meaſure engaged in the preſent Undertaking.

For, as I had reſolved to render this Courſe as compleat as I poſſibly could, both by large Models of different Fronts of Fortification, and their different Attacks, and by an

experimental Exemplification of the Precepts of Gunnery with real Artillery; I found it neceſſary to inſert under this laſt Head a Theory of the Force of Gunpowder, and certain Propoſitions relating to the Reſiſtance of the Air, which I had diſcovered, and confirmed by Experiments. But theſe Principles being ſet down in the Schemes, which I delivered out, as Aſſertions only, without any account of the Nature of the Experiments made uſe of for proving them, and being liable to great Conteſtation on account of their Inconſiſtency with all the received Opinions of the Writers on this Subject; I thought it incumbent on me to clear up in a more particular Manner any Difficulties, which might have ariſen about them, and to evince their Certainty by a number of unqueſtioned Experiments. And this has principally given Riſe to the enſuing Treatiſe, in which the Force and varied Action of Powder is ſo far determined, that the Velocities of all kinds of Bullets impelled by its Exploſion, may be thence computed, and the enormous Reſiſtance of the Air to ſwift Motions (much beyond what any former Theories have aſſigned) is likewiſe aſcertained. And on theſe Principles it will appear, that the Original Velocities of Bullets, when impelled by full Charges of

of Powder, and the Track deſcribed by their Flight, are extremely different from what, the Writers on theſe Subjects have hitherto ſuppoſed.

As the principal Diſquiſitions of the following Sheets relate to the Force of Powder, and the Flight of Shells and Bullets; it may not perhaps be unacceptable to the Reader to peruſe a few Particulars, relating to the Invention of Powder, and the Hiſtory and Improvements of Gunnery, and its Siſter Art Fortification; eſpecially as the Nature and Purport of what, we ſhall hereafter advance, will receive ſome kind of Illuſtration, by being compared with the Opinions, which have formerly prevailed in theſe Enquiries. And tho' our immediate View is the promoting the Theory and Practice of Gunnery, yet the preſent Methods of fortifying are ſo connected with the Invention and Management of Artillery (theſe Arts having in ſome Meaſure given Laws to each other) that I preſume a ſhort Recital of the Riſe and Changes of the modern Military Architecture, will not be impertinently prefixed to an Account of thoſe powerful Machines, which firſt gave it Birth.

With regard to the firſt Invention of Baſtions, there are many Opinions amongſt Au-

thors, it being as yet a Point undecided in what Place, and at what Time they were firſt put in Practice. Some have attributed this Invention to *Ziſca* the *Bohemian*. Others to *Achmet Baſhaw*, who having taken *Otranto* in the Year 1480, fortified it in a particular Manner, which is ſuppoſed to be the firſt Inſtance of the Uſe of Baſtions (*a*). But theſe are the Poſitions of later Writers. Thoſe, who wrote on the Subject of Fortification near two Centuries ago, ſeem to ſuppoſe, that Baſtions were a gradual Improvement in the ancient Method of Building, rather than a new Thought, that any one Perſon could claim the Honour of. *Paſino* in particular, in the firſt Part of his Book, imputes the Changes in the ancient Fortifications, and the Introduction of the modern Form, to the increaſed Violence of the later Artillery, without pretending that it was effected at one Time, or by one Perſon (*b*).

(*a*) *Vid.* The Commentary of the Chevalier *Folard* on *Polybius*. Tom 3. pag. 2.

(*b*) Vid. *Diſcours ſur pluſieurs poincts de l'Architecture de Guerre concernants les Fortifications, tant anciennes que modernes*, &c. *Par M.* Aurelio de Paſino Ferrarois, *architecte de tres-illuſtre Seigneur, Monſeigneur le Duc de* Buillon. Printed by *Plantin* 1579. It appears by a Copy of Verſes prefixed to the Book, that this Author fortified *Sedan*.

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So that I believe we cannot with Certainty affirm more in Relation to the Invention of Baſtions, than that they were well known ſoon after the Year 1500. For in 1546, *Tartalea* publiſhed his *Queſiti & inventioni diverſe*, in the ſixth Book of which he mentions, that whilſt he reſided at *Verona* (which muſt have been many Years before) he ſaw Baſtions of a prodigious Size; ſome finiſhed, and others building; and there is beſides, in the ſame Book, a Plan of *Turin*, which was then fortified with four Baſtions, and ſeems to have been compleated ſome time before.

And tho' we cannot certainly aſſign the Time, when the old circular Towers were firſt converted into Baſtions, yet in all Probability it did not long precede the Date, we laſt mentioned. For in the ſame Book the Prior of *Barleta*, who was himſelf a Soldier, eſteems *Turin* to be impregnable; and tells us, that this was the general Opinion of all Men of Skill; he likewiſe makes it a Queſtion, if in the fortifying of Cities, the Genius of Mankind was not arrived at its utmoſt Limits of Perfection; which ſeems to evince that the Invention was a recent one; and that it was greatly the Object of the Eſteem and Conſi-

deration of his Cotemporaries, as a new Contrivance of this kind would naturally be.

THE firſt Baſtions, ſuch as thoſe of *Turin*, of *Antwerp* (*a*), and others of the ſame Age, were but ſmall, and removed at a great Diſtance from each other; for at that Time it was the univerſal Practice to attack the Curtain, and not the Baſtions. But a few Years after there were introduced Baſtions much larger, and much nearer together, than what had been conſtructed before; as appears by the Citadel of *Antwerp*, which was built under the Direction of the Duke *D'Alva*, about 1566, and which by the frequent Encomiums on it in ſome early Authors, ſeems to have been the firſt Inſtance of this Improvement.

FROM this Period, the modern Practice of military Architecture may be ſuppoſed to have taken its Riſe; moſt of the Improvements of the preſent Times, being little more than the putting in uſe ſuch Methods, as were propoſed within a few Years of this Æra; for many celebrated Authors flouriſhed ſoon after, as (*b*) *La Treille*,

(*a*) *Antwerp* was fortified about the Year 1540, as we learn from *Speckle*. Lib. I. chap. 10.

(*b*) Vid. La Maniere de fortifier Villes, Chaſteaux, et faire autres lieux forts: mis en François par le Seigneur *de Bereil*

Treille, *Alghiſi*, *Marchi*, *Paſino*, and above all *Speckle*, (*a*) who was one of the greateſt Genius's that has applied to this Art.

THE better to judge of the Pretenſions of the Moderns, and the Merit of the Syſtems of Fortification now in vogue, we muſt enter into a ſhort Diſcuſſion of the various Methods, which have been propoſed for covering the Flanks, and conſequently for ſecuring the Ramparts from the Approach of an Enemy. For if it be agreed, that the principal Defence of a Fortreſs is its Flanks; the beſt Standard to judge of the Merit of any Syſtem of Fortification, is the Manner, in which it provides for the Safety of the Flanks, againſt the Efforts of the Enemy.

Now the moſt uſual Contrivances for this Purpoſe, have been Orillons, Ravelins placed before the Curtains, Half-Moons placed before

roil François *de la Treille* Commiſſaire en l'Artillerie. A *Lyon* 1556. This Author was the firſt I have ſeen, who propoſed the Retired Curtain, which has ſince been publiſhed by others, under the Name of the re-inforced Order.

(*a*) *Daniel Speckle* was Architect of the City of *Straſbourg*; he died in the Year 1589. He publiſhed a Treatiſe of Fortification in *German*, which was re-printed at *Leipſic* in the Year 1736.

the

the Points of the Baſtions, and Contregards, each of which we ſhall ſeparately conſider, both as to their Uſe and Antiquity.

THE Orillon is as old as the Baſtion, ſince in *Turin* and *Antwerp* (mentioned above) there is a lower Flank, which is cut out of the Subſtance of the Baſtion, and has thereby a Shoulder of a conſiderable Thickneſs, to ſcreen it from the Field-Batteries. But beſides this, the Drawings of *Paſino*, *Speckle*, *&c.* abound with Orillons of the ſame Form with thoſe now uſed, the only Difference being, that the Modern ones are leſs maſſive than the Antient ones. This Invention has had the good Fortune to ſtand its Ground in almoſt every Syſtem, which has prevailed; although it be rather on the Fame of the Services, it has formerly done, than for any Advantages the Moderns have received from it. For in ancient Sieges, it was the Cuſtom for the Beſieged to have a Retrenchment behind the Breach, by which Means the Beſiegers were obliged to lodge themſelves on the Ruins of the Breach, in order from thence to batter the Retrenchment. In this Caſe the Piece or Pieces of Artillery, which being covered by the Orillon, could not be diſmounted, were of wonderful Service to the Beſieged.

And

And many Inſtances might be given, where the Enemy have been hereby ſo gauled, after they had lodged themſelves in the Ruins of the Breach, that they have deſiſted from their Enterprize. But as it is now no longer the Faſhion to hold out, after a Breach is made in the Body of the Place, and the Ditch is near filled up, we rarely hear in the preſent Times of any great Feats performed by the Orillon.

THE Ravelins placed before the Curtains, (or Half-Moons as they are called in the modern Syſtems) were intended to protect the Flanks from croſs Shot, and to confine the Batteries, which ſhould be raiſed againſt the Flanks to the oppoſite Part of the Counterſcarp only, where they would be more expoſed to the Beſieged, and more difficult to preſerve. This Invention likewiſe is nearly as ancient as the Art of Fortification; it being to be found in great Numbers of old Places, and in almoſt every old Writer, and is ſtill continued in moſt Fortifications.

BUT the ancient Writers, whoſe principal Care was the ſecuring of their Flanks, did not rely ſolely on the Advantages, they received from the laſt-mentioned Invention. For though by

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that Means the Batteries for deſtroying the Flank were confined to one Place, yet they found on Examination, that on that Place the Enemy would have more room than was ſufficient for erecting of his Counter-Batteries; and therefore they added Half-Moons before the Points of the Baſtions; theſe were intended to poſſeſs the Ground, to which the Enemies Batteries againſt the Flanks were already confined, and thereby to render the Conſtruction of thoſe Batteries ſtill more difficult. However, they did not compleatly anſwer this Purpoſe, and have been long ſince laid aſide.

The Intention of Contregards, *(a)* which are likewiſe very Ancient, is the ſame with that of the Half-Moons laſt mentioned, that is, the Protection of the Flanks, to which Purpoſe (if properly conſtructed) they are moſt wonderfully adapted; for the Enemy in order to ruin the Flank, muſt either plant his Counter-Battery on the Contregard itſelf, which, if the Contregard be of a proper Profile, it will be impoſſible for him to do, or he muſt demoliſh a

(a) Paſino, whom we have mentioned above, claims the Invention of Contregards, tho' they were afterwards much mended by *Speckle*. But the Contregards of this Author were not before the Baſtions only, but ſurrounded the whole Place.

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Part of the Contregard to enable his Battery on the Contreſcarp to view the Flank, which is a tedious Work, attended with great Hazard and Difficulty. The ſame Inconveniency likewiſe attends him, when he would batter in Breach.

BUT notwithſtanding the Excellence of this Invention, it has been almoſt entirely neglected in the modern Syſtem of a neighbouring Nation. There have indeed been two or three Places fortified by the *French*, in which there are Pieces called by them Contregards; but they have nothing but the Name in common with thoſe, we here treat of. However their Experience of the Efficacy of this Work at *Turin* may poſſibly have induced them to think more favourably of it. For I have lately ſeen them adding Contregards to the old Works of a very conſiderable Frontier, although it was before eſteemed one of their compleateſt Places.

FROM all that we have ſaid then, it appears, that the Covering of the Flanks, was a Subject much more attended to by the ancient Engineers, than by thoſe, who have ſucceeded them; and conſequently that the Art of Fortification has not received from the

Moderns

Moderns thoſe great Improvements, which unſkilful Writers would ſometimes perſwade us to believe: For indeed in the ſecuring of the Flanks conſiſts the greateſt Strength of a Fortreſs; ſince, though all the other Defences by being expoſed to the Field-Batteries of the Enemy ſhould be ruined, yet as long as the Flanks are entire, the Rempart of the Place cannot be approached by the Enemy: And therefore, ſince this Circumſtance hath been ſo little heeded by ſome amongſt the Moderns, it muſt be owned, that the true Principles of this Art have been very imperfectly comprehended by them. For it has often happened, that they have diſputed about a few Fathoms in the Length of a Flank, a Face, or a Curtain, or a few Degrees in the Magnitude of a particular Angle; when at the ſame Time they have too much diſregarded this moſt important Conſideration of all, the ſcreening of the Flanks from the Batteries of the Enemy.

But this neglect hath been ſometimes owing to the Authority of erroneous Maxims, one of which in particular is, that, *(a)* whatever ſees, is itſelf ſeen; whence it has been inferred, that,

(a) See this Maxim urged with this View in *Pagan*'s Fortification, Chap. iv.

if

if the Flank can ſee the Enemy, the Enemy can ruin the Flank with his Batteries. But the Fallacy of this Reaſoning lies here, that the Flank if properly covered cannot ſee the Enemy, when he is in a Situation, where it is poſſible for him to raiſe Batteries, but only when he gets in a Place, where he muſt be expoſed to the Fire of the Flank without having it in his Power to return it. For Inſtance, a Piece of Cannon covered by an Orillon in the common Manner cannot be ſeen by the Enemy, till he is got over the greateſt Part of the Ditch, or is mounting the Breach, in either of which Places it is impoſſible for him to raiſe a Counter-Battery: And the more compleat the Artifice is, by which the Flank is ſcreened, the greater will be the Space, in which the Enemy will be thus expoſed.

OTHER Engineers have endeavoured to undervalue this Art as ineffectual, and with this view they have expatiated much on the Force of the modern Methods of Attack, and have declared, that no place how artfully ſoever conſtructed can ſtand before them. With theſe Gentlemen it is a Maxim, that when the Contreſcarp is once loſt, the whole Conteſt is in a manner over; and they endeavour to ſupport themſelves

themſelves in this Perſwaſion by the Examples of Places of great Note, which have been reduced in a much ſhorter Time, than was expected. If theſe Opinions could be relied on, the greateſt Part of the Money laid out in Fortifications would be extreamly ill employ'd, ſince a ſimple Rempart and a Contreſcarp would fully anſwer the whole Purpoſe intended. But the Truth is, that, when a Place is well conſtructed, and ſkilfully defended, the taking of the Contreſcarp is but a ſmall Step towards the Poſſeſſion of the Place (*a*). Indeed the Raſhneſs and Precipitancy of the Director of the Approaches, hath often intimidated a weak and ignorant Governor; but when the Attacks have been thus eagerly hurried on againſt a Place commanded by a brave and knowing Officer, he has ſometimes taken ſuch Advantages of theſe incautious Steps, as have made them too fatal to be copied by any pretending to Prudence or Humanity. By this means the eaſieſt Enterprizes have been often rendered impoſſible, and the Pretence of

(*a*) In the laſt memorable Siege of *Barcelona*, the Loſs of the Contreſcarp (which was taken in a Fortnight) did not determine the Fate of the Town, the great Reſiſtance being after the Body of the Place was opened by ſeveral Breaches.

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gaining a Day or two has often occaſioned the loſs of the whole (*a*).

Besides, thoſe Inventions for ſcreening of the Flanks, which we have already mentioned, there have been others propoſed of a different Nature, which by reaſon of their Singularity, have been leſs attended to; ſuch is the Conſtructing of a Line, which ſhould paſs through the Ditch, from the Point of the Baſtion to the oppoſite Point of the Contreſcarp. This is mentioned by General *Montecuccoli* in his Memoirs, as a Method much leſs liable to Exception, than it appears to be at firſt Sight (*b*). But though a Line thus conſtructed, will doubtleſs cover the Flanks from the View of the Batteries placed on the oppoſite Part of the Contreſcarp, and is it ſelf very defenſible,

(*a*) Many Inſtances of the Difficulties and Hazards, to which the Allies were often expoſed in *Flanders*, during the late War, may be ſeen in *Landsberg*, who was then an Engineer in the Service of the States-General; theſe Accidents, according to him, were generally owing to the Preſumption of the Directors, who under the Pretence of Expedition, contracted the Front of their Attacks, and thereby often left the Enemies Works in their Rear, which rendered their Progreſs next to impoſſible. Vid. *Nouvelle Maniere de Fortifier les Places.*

(*b*) Vid. Memorie del general Principe di Montecuccoli, pag. 116.

yet I have never heard of its being put in Execution.

ANOTHER Way of ſecuring the Flanks, is by interpoſing the Entring Angle of the Contreſcarp, (or of the Ravelin) between them and the Counter-Batteries. This Practice is deſcribed by *Errard* of *Barleduc*, *(a)* and is by him ſaid to be the Invention of the Count of *Lynar*. And though ſome Authors, who were ignorant of the Advantages hereby propoſed, have ſeverely cenſured the having any Part of the Ditch hidden from the Flank, a Circumſtance which muſt neceſſarily attend this Conſtruction; yet the greateſt Genius, who ever applied himſelf to the Study of this Art, has thought it worthy of his Imitation; the celebrated Fortreſs of *Bergben-Op-Zoom*, having its Flanks in part covered by this Artifice.

BUT in a proper Soil there is ſtill a more efficacious Defence, than any we have yet men-

(a) Vid. *La Fortification demontré*, Lib. iii. Chap. ii. Beſides this Invention here mentioned, there occurs in this Author, the Contrivance of placing a Gallery under the Cover'd-Way, with Loop-Holes into the Ditch, which is practiſed at *Tournay*, but more compleatly at *Bergben-Op-Zoom*. Vid. Lib. iv. Chap. vii.

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tioned; and that is, by the Means of Contre-mines. For ſuppoſing the Fortifications of a Place to be conſtructed with no more Art, than what is neceſſary to oblige an Enemy to bring his Batteries on the Glacis, when he propoſes either to batter in Breach, or to ruin the Flanks, (which may be effected by a good Profile and a Ravelin before the Curtain only.) If the Soil be free from Water to a conſiderable Depth, it is always in the Power of the Beſieged to ruin the Batteries of the Enemy by their Mines, which may be repeated too a number of Times, in Proportion to the Depth of the Soil. For theſe Batteries being by Suppoſition confined to one Situation, the Beſieged can always be prepared for theſe Operations before-hand, and would have infinite Advantages over an Enemy, who ſhould endeavour to dig them out; which, however in ſuch Circumſtances, would be his only Reſource.

The firſt ſucceſsful Application of the Blowing of Mines in Sieges, was in the Kingdom of *Naples*, where *Pietro de Navarre* by this Means poſſeſſed himſelf of a Fort garriſoned by the *French*. But the firſt celebrated Uſe of theſe Mines in oppoſing the Progreſs of the Beſiegers, was in the Years 1666, 67, 68, at the Siege of

Candia; not but they had been often practiſed in the Defence of Places before, though in a leſs memorable Manner; for by the Aſſiſtance of this Invention principally, the City of *Candia* kept the whole Power of the *Ottoman* Empire at a Bay for three Years ſucceſſively. Since that Time the Advantages of Contremines have been better underſtood. The laſt eminent Inſtance of their great Uſefulneſs, was in the Defence of *Turin*, in the Year 1706; for ſo effectually were the Beſiegers traverſed thereby, that after near four Months of open Trenches, they were not in the Poſſeſſion of more than the Contreſcarp, and even there, eleven Pieces of their Cannon were blown up by the Defendants, but three or four Days before the Place was relieved.

Before I leave this Head, I cannot but in Juſtice mention the great Improvement in the Doctrine of Mines, which is contained in that excellent Diſſertation (*a*) annexed to the third Volume of the *French Polybius*. For nothing can be more compleat, than the Manner in

(*a*) This in the Preface, is ſaid to be the Performance of Monſieur *de Valiere*, Marechal des Camps, and Captain-General of the Miners.

which

which the different Stages of Mines are there diſtributed; indeed, the Form there aſſigned to the Excavation, cannot be rigorouſly, what the Author ſeems to ſuppoſe; but this Exception has nothing to do with his general Rangement of the Chambers, which is extremely well contrived for the Huſbanding of the Ground, and the Annoyance of the Enemy.

I HAVE already taken notice of the Defects in the Writings of many of thoſe, who amongſt the Moderns have undertaken to form Syſtems of Fortification. But when I ſpeak of theſe Authors and their Copiers, I muſt at the ſame Time avow the ſuperior Merit of the great *Coehoorn*, who was undoubtedly the ableſt Fortifier, that ever the World knew. This Author has publiſhed two Treatiſes on this Subject; the Firſt containing a Method of Fortifying a Pentagon, to which is annexed a Project for the Amending the Fortifications of *Coevoerden*. In his Second, he has propoſed three different Manners of Fortifying, one applied to a Hexagon, another to a Heptagon, and a third to an Octagon; and he has beſides added, the Manner of Fortifying that Side of a Fortreſs, which happens to be contiguous to a River. In this Work he has particularly examined all the poſſible

Attacks, that can be formed againſt his propoſed Places, thereby to evince the great Superiority of his Defences; ſo that it is in ſome Meaſure a Diſcourſe on the Attack and Defence of Places, as well as a Syſtem of Fortification; and upon the Whole is the moſt excellent Performance, that has ever been produced on this Subject. It was written in *Low-Dutch* (the Author's Native Language) but has been tranſlated into both *French* and *Engliſh*, but very imperfectly; tho' in a new Edition of the *French* Tranſlation lately printed in *Holland*, many of the Errors of the former are amended, and ſome particular Paſſages are cleared up by the Notes of the Editor, who ſeems to have underſtood his Author very well.

I HAVE been told by thoſe, who were well acquainted with this great Man, that his Treatiſes were far from acquiring him either the Advantages or Reputation, which he might reaſonably have hoped from them. For that his Cotemporary Engineers, wedded to their old Road, decried him as an unſkilful, ſelf-conceited Pretender; but that he at laſt ſurmounted theſe Effects of their Envy and Prejudice by his Defence of Fort *William* at *Namur*, when that Place was beſieged by the *French*; after

after this, which eſtabliſhed his Reputation, he roſe apace to the greateſt Military Commands, and immortalized his Name by his Conduct of the Siege of *Namur* under King *William*, and afterwards at *Bon*, *Limburg*, the Citadel of *Liege*, &c. And his Death at the Beginning of the late War in *Flanders* was a very great Misfortune to the Allies, of which almoſt every Siege formed by them after the Year 1707, was a melancholy Proof.

Besides being entruſted with the Direction of Sieges, he was employed too in the repairing and new-modelling many of the *Dutch* Frontiers. His laſt work, which is left unfiniſhed, was *Bergen op Zoom*, which will always do Honour to his Memory. Tho' he is yet ſo little out of the Reach of Cenſure, that I have heard Military Men, even in that Place, condemn as Imperfections, thoſe very Circumſtances, whence it derives its principal Defence.

Considering the great Fame which General *Coehoorn* acquired in real Service, it is difficult to account for the little Regard which hath been paid to his Writings. The moſt natural Reaſon I can diſcover for this Negli-

gence, is the Proneneſs, which we have always ſhewn for the Opinions of a neighbouring Nation, who, whatever other good Qualities they may have, were never famous for doing juſtice to the Merit of thoſe, who were of another Country, and were engaged in an Intereſt oppoſite to their own. However, I preſume his Reputation as an Author is at preſent increaſing. For I ſaw not long ſince in one of the moſt conſiderable Frontiers belonging to *France*, a Piece of Fortification carrying on, which was evidently copied from the printed Works of *Coehoorn*.

THOUGH with Regard to the modern Writers on Fortification, I cannot find another to place in the ſame Article with the great Genius, laſt mentioned; yet there are two Authors on the Methods of attacking and defending Places (a Subject nearly connected with Fortification) who merit the higheſt Applauſe, I mean *Goulon* and the Marechal *de Vauban*. The firſt in a ſhort Treatiſe, intitled *Memoires ſur l'Attaque et la Defence des places*. In which he has very diſtinctly inculcated the principal Maxims neceſſary in thoſe Operations. The other in a Work which he preſented in

Manuſcript

Manuſcript to the late King of *France*, of which Copies getting Abroad, it was publiſhed four Years ſince in *Holland*. In this Book Mr. *Vauban* has very circumſtantially deſcribed thoſe Parts of the Attack, which were more immediately of his own Invention; ſuch as the *Batteries à Recochet*; the Parallels, and a peculiar Conduct of the Sap. Not but that he has likewiſe given very ample Inſtructions on every other neceſſary Head; and the whole muſt be owned to be a very maſterly Performance, worthy of the Experience and Capacity of its great Author.

It might, perhaps, be expected, that I ſhould here mention with Approbation the Skill of this laſt-mentioned Engineer in the Art of Fortifying. But as he has never written any Thing himſelf on this Subject, that may excuſe me from ranging him in the Liſt of Authors. Bnt to ſpeak the Truth on this Head, I cannot but believe from all, I have hitherto ſeen of his Works, that he was much more to be eſteemed for his other Talents than for the Fortifications he has erected. For though I have a very high Opinion of his good Senſe and Diſcernment; I do not conceive that his

his Invention in this Art was to be compared with that of his Cotemporary *Coehoorn*.

THUS much may ſuffice on the Origin and Variations of the preſent Military Architecture. We muſt next diſcuſs what is more immediately connected with the Purport of the enſuing Treatiſe, I mean the Invention of Powder and Artillery, with their reſpective Improvements, and the different Theories, they have given Riſe to.

THE Invention of Gunpowder is uſually aſcribed to one *Bartholdus Schwartz*, a *German* Monk, who diſcovered it, as is ſaid about the Year 1320, and the firſt Uſe of it in War is commonly ſuppoſed to have been by the *Venetians* againſt the *Genoeſe* about the Year 1380. But both theſe Suppoſitions are undoubtedly falſe; for a Compoſition reſembling that, which we call Powder, is mentioned by *Roger Bacon* as well known in his Time, and he lived near fifty Years before *Schwartz*; and there are indiſputable Proofs of the Uſe of Artillery much earlier than the Year 1380.

INDEED, as the Time of the Diſcovery of Saltpetre is confeſſedly uncertain, it is not to be

be wondred at, that the Invention of Gunpowder ſhould be obſcure and unknown; for theſe two Diſcoveries are ſo connected, that it is difficult to conceive, how the firſt could be long known, before the latter was found out.

THE diſtinguiſhing Property of Saltpetre is the prodigious Increaſe of Inflammability, which it produces in all burning Subſtances, when mixed with them; altho' alone and unmixed it will neither flame, nor burn. For Inſtance, Saltpetre put into a Crucible, and placed in the hotteſt Fire will only melt, and grow red hot, but will neither explode nor flame; yet, if any inflammable Subſtance (Sulphur ſuppoſe, or Coals) be thrown into it, a violent Blaze will be inſtantly produced, in which a Part of the Saltpetre will be conſumed in Proportion to the Quantity of the inflammable Subſtance, which was put to it. And a like Exploſion will take place, if Saltpetre be thrown upon any Fire. Now it cannot be reaſonably ſuppoſed, that this Quality of Saltpetre could be long unknown, after the Subſtance itſelf was diſcovered; for the accidental dropping of any ſmall Part of it into the Fire, would prove its prodigious exploſive Power when mixed with burning Bodies. And this

this being once obſerved, it was thence a very natural and obvious Thought to invent a Compoſition of Saltpetre mixed with any inflamable Subſtance which would burn more violently than any known before. And our prepreſent Gun-Powder is only the Improvement and Perfection of ſuch a Mixture.

On this Suppoſition then, if we knew the Time, when Saltpetre firſt came in uſe, we might give ſome gueſs, when Mixtures reſembling our preſent Gun-Powder were firſt invented. Now the moſt general Opinion on this Head is, that Saltpetre was firſt diſcovered, either by the *Arabians* or the later *Greeks* about the middle Ages of our Æra, when Alchymy and Chymiſtry were eagerly purſued by both Nations. For its *Arabick* Name is ſaid to be expreſſive of its exploſive Quality; and the *Greek* Fires uſed in War by the later *Greek* Emperors (if the Effects attributed to them by many Authors are true) muſt have had Saltpetre in their Compoſition.

Indeed ſome Moderns (miſled by a Similarity of Names) have ſuppoſed Saltpetre or Nitre to have been known to the Ancients. But Chymiſts are now agreed, I think, that the

the Subſtance mentioned by ſome ancient Writers, and deſcribed by *Pliny*, by the Appellation of Nitre, is a Salt altogether different from what we call Saltpetre.

Now, that the firſt Invention of Gun-Powder (or of Compoſitions reſembling it) did long precede the Time of *Schwartz* or of *Bacon*, and may thence be reaſonably ſuppoſed nearly coeval with the Knowledge of Saltpetre, appears from *Bacon* (*a*) himſelf; for it is not a new Compoſition, which he propoſes, but the Application of an old one to military Purpoſes. And from his Words it plainly appears, that a Mixture of Saltpetre with other Sub-

(*a*) *Bacon* tells us, that Sound like Thunder, and Lightnings greater than thoſe produced by Nature, might be made by Art; and this many Ways, by which a City or an Army might be deſtroy'd; and he ſuppoſes it to be by an Artifice of this Kind, that *Gideon* defeated the *Midianites*: And having in another Treatiſe mentioned almoſt the ſame Thing in different Words, he adds, *Et experimentum hujus rei capimus ex hoc ludicro puerili quod fit in multis mundi partibus, ſcil. ut inſtrumento facto ad quantitatem pollicis humani ex violentia illius ſalis, qui* SAL PETRÆ *vocatur, tam horribilis ſonus naſcitur in ruptura tam modicæ rei, ſcil. modici pergameni, quod fortis tonitrui excedere rugitum, & corruſcationem maximam ſui luminis jubar excedit. Vid.* Doctor *Jebb's* Preface to his Edition of *Bacon's Opus Majus.*

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ſtances was then vulgarly uſed for the making of recreative Fire-works. And this appears yet plainer from the Treatiſe of *Marcus Græcus*, intitled, *Liber ignium* (*a*); for this Author deſcribes two kinds of Fire-works, one for flying, and the other for making a Report. The Caſe or *Cartouche* (*Tunica*) for the firſt, he directs to be made long and ſlender, and the Compoſition to be very cloſe rammed; the Caſe for the ſecond he orders to be thick and ſhort, to be ſtrongly tied at both Ends, and to be but half filled; and the Compoſition he preſcribes for both is two Pound of Charcoal, one Pound of Sulphur, and ſix Pound of Saltpetre, well powdered and mixed together in a Stone Mortar: And this will be allowed to be a ſtronger Compoſition, than what great Quantities of Powder are every Day made with. Now tho' the Age of this Writer is not well aſcertained, yet it muſt have preceded the uſe of Artillery; for he does not in any Place (as I can learn) mention theſe Compoſitions as uſed in War; and as he pretends not to be the Inventor of theſe Serpents or Crackers (for ſuch we ſhould now call them) nor

(*a*) This is a Manuſcript in the Poſſeſſion of Dr. *Mead*; but what is here mentioned is quoted by the Editor of *Bacon*'s *Opus Majus* in the Preface.

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ſpeaks of them as recent, we may reaſonably preſume, they were in uſe long before his Time.

THE firſt Application of this Mixture to Military Affairs, ſeems to have been ſoon after the Year 1300. *Bacon*'s Propoſal (which was about the Year 1280,) to make uſe of its enormous Exploſion for the Deſtruction of Armies might give the firſt Hint, which others might afterwards purſue. *Schwartz* inſtead of being the firſt Inventor of Gun-Powder, might poſſibly be one of the firſt, who thus apply'd it; and indeed the common Account of the manner, in which he came at his Invention, very much favours this Opinion *(a)*. And perhaps the different Improvements

(*a*) The uſual Manner in which it is told is, that *Schwartz* having pounded the Materials of Gun-Powder in a Mortar, which he afterwards covered with a Stone, a Spark of Fire accidentally flew into the Mortar, and the Exploſion blew the Stone, which covered the Mortar, to a conſiderable Diſtance. Now we have proved, that *Swhartz*, who was a Chymiſt, could not diſcover the Compoſition by this Means, becauſe it was commonly known before; but he might from hence be taught the ſimpleſt Method of applying it in War: For *Bacon* ſeems rather to have conceived the Manner of uſing it to be by the actual Effort of the Flame againſt the Bodies it might meet with in its Expanſion.

ments ſoon added by others, or the Proſecution of *Bacon*'s Thought in different Places, may have given Riſe to the different Dates aſſigned by Hiſtorians for the firſt uſe of Artillery.

GUN-POWDER for ſome Time after the Invention of Artillery, was of a Compoſition much weaker *(a)* than what we now uſe, or than that ancient one mentioned by *Marcus Græcus*; but this, I preſume, was owing to the Weakneſs of their firſt Pieces, rather than to the Ignorance of a better Mixture. For the firſt Pieces of Artillery were of a very clumſy, inconvenient Make, being uſually framed of ſeveral Pieces of Iron fitted together lengthways, and then hoop'd with Iron Rings; and as they were firſt employed in throwing Stone Bullets of a prodigious Weight, in Imitation of the ancient Machines, to which they

ſion. The Figure and Name of Mortars given to a Species of the old Artillery, and their Employment (which was throwing great Stone Bullets at an Elevation) very much corroborate this Conjecture.

Vide Tartalea in his *Queſiti & Inventioni*, Lib. 3. *Queſito* 5. where there are ſet down twenty-three different Compoſitions made uſe of at different Times. The firſt of which being the moſt ancient, contains equal Parts of Nitre, Sulphur, and Charcoal.

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ſucceeded, they were of an enormous Bore. But the Difficulties of conducting and managing theſe cumbrous Pieces, and the diſcovering that Iron Bullets of much leſs Weight than Stone ones, would be more efficacious, if impelled by greater Quantities of ſtronger Powder, ſoon occaſioned an Alteration in the Matter and Fabric of theſe firſt Pieces, and gave Riſe to what we ſtile Braſs Cannon, which though lighter and more manageable, were yet much ſtronger in proportion to their Bore, by which Means they would endure great Charges of a better Powder, than what had firſt been uſed, and their Iron Bullets (which were from forty to ſixty Pound Weight) being impelled with greater Velocities were more effectual, than the weightieſt Stones could ever prove *(a)*.

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(a) The Time when this Change took Place, and the Advantages ariſing from it, are mentioned by *Guicciardin*, who ſpeaking of the *French* Army intended for the Invaſion of *Italy*, in the Year 1494, ſays,

——— *Et per unirſi con queſto eſercito erano ſtate condotte per mare a* Genoua *quantità grande d' artiglierie da battere le muraglie, & da uſare in campagna, ma di tale ſorte, che giamai non haveva veduta* Italia *le ſimiglianti. Queſta peſte trovata molt' anni innanzi in* Germania, *fu condotta la prima volta in* Italia *da* Vinitiani *nella guerra, che circa l'anno della*

By this Means, Powder compounded in the ſame Manner, which is now practiſed by

della ſalute 1380, *hebbono i* Genoueſi *con loro.* ——— *Il nome delle maggiori era bombarde. le quali, ſparſa dopo queſta inventione per tutta* Italia *s'adoperavano nell' oppugnatione delle terre, alcune di ferro, alcuni di bronzo, ma groſſiſſime, in modo che per la machina grande, & per l' imperitia de gl' huomini, & mala attitudine de gl' inſtrumenti tardiſſimamente & con grandiſſima difficultà ſi conducevano, piantavanſi alle terre con medeſimi impedimenti, & piantate, era dall' un colpo all' altro tanto intervallo, che con piccoliſſimo frutto a' comparatione di quello, che ſeguito dopo, molto tempo conſumavano, donde i defenſori de luoghi oppugnati havevano ſpatio di potere otioſamente fore di dentro ripari & fortificationi.* ——— *Ma i* Franceſi *fabricando pezzi molti piu eſpediti, ne di altro che di bronzo, i quali chiamavono Cannoni, & uſando palle di ferro, dove prima di pietra, & ſenſa comparatione piu groſſe & di peſo graviſſimo, s' uſavano, gli conducevano ſu le carette, tirate (non da buoi, come in* Italia *ſi coſtumava) ma da cavalli con agilità tale d'huomini, & d'inſtrumenti deputati a queſto ſervigio, che quaſi ſempre al pari de gl'eſerciti caminavano,& condotte alle muraglie erano piantate con preſtezza incredibile, interponendoſi dall' un colpo all' altro piccoliſſimo intervallo di tempo, ſi ſpeſſo, & con impeto ſi gaggliardo percotevano, che quello che prima in* Italia *fare in molti giorni ſi ſoleva, da loro in pochiſſime hore ſi faceva. Vid.* Guicciardin's Hiſtory, L. 1. p. 45. What this Author obſerves of the prodigious Size of the Stone Bullets uſed whilſt the old Pieces were in Faſhion, will be better underſtood by knowing, that when *Mahomet* the ſecond beſieged *Conſtantinople* in the Year 1453, he battered the Walls with Stone Bullets, and his Pieces were ſome of them of the Calibre of 1200 Pounds; but then they could not be fired more than four Times a Day.

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all *Europe*, came in uſe (*a*). But the Change of the Proportion of the Materials compoſing it, was not the only Improvement it receiv'd. The Invention of graining it, is doubtleſs a conſiderable Advantage to it. For Powder at firſt was always in the Form of fine Meal, ſuch as it was reduced to, by grinding the Materials together. And it is doubtful, whether the firſt graining of Powder was intended to increaſe its Strength, or only to render it more convenient for the filling into ſmall Charges, and the charging of ſmall Arms, to which alone it was applied for many Years, whilſt Meal-Powder was ſtill made uſe of in Cannon. But at laſt the additional Strength, which the grain'd Powder was found to acquire from the free Paſſage of the Fire between the Grains,

(*a*) We learn from *Tartalea*, that the Cannon Powder in his Time *(polver groſſa moderna)* was made of four Parts Saltpetre, one Part Sulphur, and one Part Charcoal, and the Muſquet Powder of forty-eight Parts Saltpetre, ſeven Parts Sulphur, and eight Parts Charcoal; or of eighteen Parts Saltpetre, 2 Parts Sulphur, and three Parts Charcoal. Theſe Compoſitions for Muſquet Powder are very near the preſent Standard; the firſt having in one hundred Pounds of Powder, about one Pound of Saltpetre more than is at preſent allowed, and the ſecond three Pounds more.

occaſioned the Meal-Powder to be entirely laid aſide *(a)*.

THE Formation of Artillery hath been very little improved in the laſt two hundred Years; the beſt Pieces now caſt, not differing greatly in their Proportions from thoſe made in the Time of the Emperor *Charles* V. Indeed lighter and ſhorter Pieces have been often propoſed,

(*a*) That Powder was firſt uſed in Meal; and that long after the Invention of Graining it for the the Uſe of ſmall Arms, Cannon-Powder continued in its old Form, are Facts not to be conteſted. *Tartalea* in his *Quæſiti*, L. 3. *Queſ.* 9. and 10. expreſsly aſſerts, that then the Cannon-Powder was in Meal, and the Muſquet-Powder grained. And our Countryman *William Bourn*, in his ART OF SHOOTING IN GREAT ORDNAUNCE, publiſhed forty Years after *Tartalea*, tells us in Chap. 1. that Serpentine-Powder (which he oppoſes to Corn or Grained-Powder) ſhould be *as fine as Sand, and as ſoft as Flour:* And in his third Chapter he ſays, that two Pounds of Corn-Powder will go as far as three Pounds of Serpentine-Powder. Alſo Sir *Henry Manwayring*, in his SEAMAN'S DICTIONARY preſented to the Duke of *Buckingham* in the Time of *Charles* I. under the Word *Powder*, tells us, *there are two Kinds of Powder, the one Serpentine-Powder, which Powder is Duſt (as it were) without corning* —— *The other is Corn-Powder.* Though he informs us the Serpentine-Powder was not uſed at Sea. Indeed when that Book was wrote, I believe, Powder was uſually corned, for the foreign Writers on Artillery had long before recommended its general Uſe.

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and eſſayed; but, though they have their Advantages, and are extremely uſeful in particular Circumſtances, yet it ſeems now to be agreed, that they are altogether inſufficient for general Service. But though the Proportions of Artillery have not been much varied within that Period, yet its Uſe and Application have undergone conſiderable Changes, the ſame Ends being now generally purſued by ſmaller Pieces, than what were formerly thought neceſſary. Thus the Battering-Pieces now univerſally approved of, are the Demi-Cannons, carrying a Ball of twenty four Pound weight, it being found by Experience, that their Stroke, tho' leſs violent than that of larger Pieces, is yet ſufficiently adapted to the Strength of the uſual Profiles of Fortification, and that the Facility of their Carriage and Management, and the Ammunition they ſpare, give them great Advantages beyond the whole Cannons formerly employed in making Breaches. The Method too (now generally followed) of forming a Breach, by firſt cutting off the whole Wall as low as poſſible, before its upper Part is attempted to be beat down, ſeems alſo to be a conſiderable modern Improvement in the practical Part of Artillery. For I do not remember to have ſeen this Procedure recommended

mended by any ancient Author; and *Gabriel Buſca* (*a*) who boaſts much of his great Experience, expreſsly directs the Contrary. Indeed *Collado* mentions it as the Practice of the *Turks*, (*b*) but it is without commending it, or propoſing it as an Example to be followed.

BUT the moſt important Improvement in the practical Management of Artillery (for of the ſcientific Part we ſhall treat by its ſelf) is the Method of firing with ſmall Quantities of Powder, and elevating the Piece ſo that the Bullet in its Deſcent may juſt go clear of the Pa-

(*a*) Vid. His *Inſtruttione de Bombardieri*, printed at *Carmagnola* in 1584, Cap. xxxvii. in which Place he orders the Breach to be begun at the upper Part of the Wall, and from thence to be continued downwards.

(*b*) Vid. *Pratica Manuale di Artegliera d'al Mag. Signor Luigi Collado Hiſpano, Bettico, Nebriſenſe*, printed at *Venice* in the Year 1586, Cap. xx. Where he ſays, —— *nelle fattioni del gran Turco —— ſempre ſi adoperano i pezzi —— da tagliare le muraglie per di ſotto di eſſe tranſverſalmente, et di poi di alto in baſſo a perpendicolo, & applicandovi poi tutti a un tratto i baſiliſchi, con che fanno caſcar giu quella parte di muraglia che era gia tagliata.* This Book here quoted was compoſed and publiſhed in *Italian*, altho' the Author was a *Spaniard*. But he ſerved as an Engineer in the *Spaniſh* Army in *Italy*. And he tells us in his Preface, that he ſoon intended to re-publiſh it in *Spaniſh*, which laſt Edition is, I preſume, what is quoted by *Blondel* in his *Art de jetter les Bombes*.

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rapet of the Enemy, and drop into their Works. By this Means the Bullet coming to the Ground in a ſmall Angle, and with a ſmall Velocity, it either bounds or rolls along in the Direction it was fired in; and therefore, if the Piece be placed in a Line with the Battery it is intended to ſilence, or the Front it is to ſweep, each Shot rakes the whole Length of that Battery or Front, and has thereby infinitely more Chance of diſabling the Defendents, and diſmounting their Cannon, than it would have, if it was fired againſt the ſame Works in the common Manner. This Diſpoſition of Artillery, which is indeed a moſt uſeful one, is the Invention of the Marechal *de Vauban*, and is by him ſtiled *Batterie à ricochet* (*a*) and was firſt put in Practice at the Siege of *Aeth*, in the Year (*b*) 1692.

AFTER this brief Recital of what has been done in the mechanic Part of Gunnery, we muſt next mention the different Theories, which have been from time to time advanced in relation to the Motions of Shells and Bul-

(*a*) Vid. his Book *De l'Attaque et la Defence des places.*

(*b*) *Vid.* The Journal of this Siege printed at the end of the laſt Edition of *Goulon's Memoires.*

lets; in which Inquiry we ſhall not indeed find many Things worthy of Approbation, or even of Attention; but however, as it is a Theme in ſome Meaſure connected with the Subject of the following Treatiſe, we muſt beg the Reader's Indulgence.

THE firſt Author I have ſeen, who has profeſſedly written on the Flight of Cannon Shot, is *Tartalea*, a celebrated *Italian* Mathematician, famous for having invented the Method of ſolving cubic Equations, which is uſually aſcribed to *Cardan*. This Author in his *Nova Scientia*, printed at *Venice* in the Year 1537, and afterwards in his *Queſiti et Inventioni diverſi*, printed at the ſame Place in 1546, has profeſſedly diſcuſſed ſeveral Particulars relating to the Theory of theſe Motions. And though the then imperfect State of Mechanicks furniſhed him with very fallacious Principles to proceed on, yet he was not altogether unſucceſsful in his Inquiries: For he is ſuppoſed to be the firſt who aſſerted that the greateſt Range of Projectiles was at an Elevation of 45°. He likewiſe determined (contrary to the Opinion of Practitioners) that no Part of the Track deſcribed by a Bullet was a right Line, although the Curvature was in ſome caſes

caſes ſo little, as not to be attended to, he comparing it to the Surface of the Sea, which though it appears to be a Plain when partially conſidered, is yet undoubtedly incurvated round the center of the Earth. He alſo aſſumes to himſelf the Invention of the Gunner's Quadrant, and has often given ſhrewd Gueſſes at the Event of ſome untried Methods which were propoſed to him. But as he had never been converſant in the Practice of Artillery, but founded his Opinions on Speculation only, almoſt all the Writers who ſucceeded him were perpetually carping at him, though often without naming him; of which many Examples might be given from the Works of *Buſca*, *Collado*, (*a*) *Uſano*, *Simienowicz*, &c. And the Philoſophers of thoſe Times often intervening in the Queſtions hence ariſing, there were hereby many Diſputes on Motion ſet on

(*a*) *Collado* cap. lxiii. denies that *Tartalea* was the Inventor of the Gunner's Quadrant, and quotes *Daniel Santbech*, or *Regiomontanus* (for he confounds them) as having known it many Years before. But the Truth is, that *Santbech*'s Book from whence his Quotation is taken (*Problematum Aſtronomicorum & Geometricorum ſectiones ſeptem*) was not printed till the Year 1561, which was long after *Tartalea*. Nor did *Santbech*, though he talks of the different Elevations of Artillery, know the Method of framing a Quadrant proper for his Purpoſe.

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foot (eſpecially in *Italy*) which continued till the Time of *Galileo*, and perhaps gave Riſe to his celebrated Dialogues on Motion, which were firſt printed in the Year 1638. And in this Interval, or before the Doctrine of *Galileo* was eſtabliſhed, many Theories of the Motions of military Projectiles, and many Tables of their comparative Ranges at different Elevations were publiſhed, all of them egregiouſly fallacious, and utterly irreconcileable with the Motions of thoſe Bodies, although ſome of them were the Labours of ſuch who had ſpent the greateſt Part of their Lives in Employments relating to the Artillery. Such were the Tables of *Ufano*, of *Galeus*, of *Ulrick*, *&c.* taken notice of by *Blondel* (*a*). To which might be added, many more not mentioned by that Author. Indeed there have been very few ancient Writers on this Subject (and they are a numerous Sect) who have not indulged themſelves in ſome Speculations on the Difference betwixt natural, violent, and mixt Motions, although in the Application of theſe

(*b*) Note, the Opinion diſcuſſed by *Blondel* in his *Art de jetter les Bombes*, Cap. v. is not originally of *Rivaltius* whom *Blondel* quotes for it, but of the laſt mentioned *Santbech*, from whom *Rivaltius* ſtole it. Vid. *Santbech*, Sect. 6.

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miſtaken Notions, ſcarce any two of them agreed.

BUT what is moſt ſtrange, is, that during theſe Conteſts ſo few of thoſe, who were intruſted with the Charge of Artillery, ſhould think it worth while to examine their reſpective Theories by proper Experiments. However thus it has happened, for I do not remember to have met with more than four Authors, who have actually tried the Ranges of Shot and Shells at different Elevations. The firſt of theſe is *Collado*, who has given us the Ranges of a Falconet carrying a three Pound Shot to each Point of the Gunner's Quadrant. But from his Numbers it is manifeſt, that the Piece was not charged with its cuſtomary Allotment of Powder *(a)*. The next is our Countryman *Bourne* in

(*a*) The Reſult of his Trials was, that the point blank Shot extended 268 Paces. At an Elevation of one Point (which is the twelfth Part of the Quadrant or $7^{\circ}\frac{1}{2}$) the Range was 594 Paces; at an Elevation of two Points, the Range was 794 Paces; at three Points, 954 Paces; at four Points, 1010 Paces; at five Points, 1040 Paces; and at ſix Points 1053 Paces. The Range at the ſeventh Point fell between thoſe of the third and fourth, at the eighth Point it fell between the Ranges of the ſecond and third; at the ninth Point it fell between the Ranges of the firſt and ſecond;

in a Treatiſe printed the next Year after *Collado.* His Elevations were not regulated by the Points of the Gunner's Quadrant, but by Degrees, and he aſcertains the Proportion between the Ranges at different Elevations, and the Extent of the Point blank Shot *(a).* But he has not informed us, with what Piece he made his Trials; tho' by his Proportions, I preſume, it muſt have been a ſmall one. It were to be wiſhed, that he had ſet down this Circumſtance; for we ſhall hereafter ſhew, that the Relation between the Extent of different Ranges will vary extremely, according to the Velocity and Denſity of the Bullet. The other

cond; at the tenth Point it fell between the point blank Diſtance and that of the firſt Point; and at the eleventh Point it fell very near the Piece. *Vid. Cap.* lxi. And note, that the Paces uſed by this Author are not geometrical Paces, but common Steps, as he informs us *Cap.* xlii.

(*a*) If 1 repreſents the Extent of the point blank Shot, then according to this Author, the Range at 5° will be $2\frac{1}{9}$, at 10° it will be $3\frac{1}{7}$, at 15° it will be $4\frac{1}{3}$ at 20° it will be $4\frac{4}{5}$, and the greateſt Random will be $5\frac{1}{2}$; which greateſt Random, he tells us, in a calm Day is at 42°; but according to the Strength of the Wind, and as it favours or oppoſes the Flight of the Shot, it may be from 45° to 36°. *Vid.* His Art of Shooting in great Ordnaunce, *Cap.* vii.

two,

two, which have occurred to me, are *Eldred* and *Anderſon*, both *Engliſhmen*; the laſt of theſe having vitiated his Experiments by his too great Attachment to an erroneous Theory, I ſhall have Occaſion to mention him hereafter. But *Eldred* (*a*) deſerves a better Character; his Principles were ſufficiently ſimple, and tho' not rigorouſly true, they were, within certain Limits, near the Truth. He has given us the actual Ranges of different Pieces of Artillery at ſmall Elevations, all under ten Degrees. His Experiments are numerous, and appear to be made with great Care and Caution; and he has honeſtly ſet down ſome, which were not reconcileable to his Method; and upon the whole ſeems to have taken more Pains, and to have had a juſter Knowledge of his Buſineſs, than is to be found in many of his practical Brethren. For they have been generally too much attach'd to ſome incorrect Theory, or to the common Uſage which they have always followed, to think of extending their Art by proper Expe-

(*a*) His Book is intitled THE GUNNER'S GLASSE, and the Experiments he relates were moſt of them made at *Dover-Caſtle*, of which Place he was many Years Maſter-Gunner. The earlieſt Date I find to any of his Experiments is 1611, but his Book was not publiſhed till 1646.

riments,

riments, or indeed to conceive, that it was not already compleat; it would otherwiſe have been impoſſible, that Poſitions ſo little to be reconciled with Experience, ſhould have held their Ground ſo long as they have done, a remarkable Inſtance of which, is the Doctrine which has taken Place in this Subject, ſince the Time of *Galileo*.

GALILEO printed his Dialogues on Motion in the Year 1638, as we have already obſerved; and in theſe he has pointed out the general Laws obſerved by Nature in the Production and Compoſition of Motion, and was the firſt, who deſcribed the Action and Effects of Gravity on falling Bodies; and on theſe Principles he determined, that the Flight of a Cannon-Shot, or of any other Projectile, would be in the Curve of a Parabola, unleſs ſo far as it were diverted from that Track by the Reſiſtance of the Air. And what Inequalities would thence ariſe, he has propoſed the Means of examining; for he has deſcribed a Method of diſcovering what ſenſible Effects that Reſiſtance would produce in the Motion of a Bullet at ſome given Diſtance from the Piece.

WHEN *Galileo* had thus ſhewn, that, independent of the Reſiſtance of the Air, all Projectiles

jectiles would in their Flight deſcribe the Curve of a Parabola, it might have been expected that thoſe who came after him, would have tried how far the real Motions of Projectiles deviated from a parabolic Track, in order thence to have decided whether the Reſiſtance of the Air was, or was not, neceſſary to be attended to in the Determinations of Gunnery. But inſtead of this cautious Procedure, the ſubſequent Writers on Gunnery have boldly aſſerted (without an experimental Examination) that no conſiderable Variation could ariſe from the Reſiſtance of the Air, in the Flight of Shells or Cannon-Shot, ſupporting themſelves in this Perſuaſion chiefly by the Conſideration of the extreme Rarity of the Air, compared with the denſe and ponderous Compoſition of thoſe projected Bodies. And hence (this Maxim of the inconſiderable Effects of the Air's Reſiſtance to the Motion of Shells and Bullets, being continually repeated and copied by ſucceeding Authors,) it is now become an Axiom almoſt generally acquieſced in, that the Flight of theſe Bodies is nearly in the Curve of a Parabola.

FOR in the Year 1674, our Countryman *Anderſon* publiſhed his Treatiſe of *the genuine Uſe and*

and Effects of the Gun. In which he proceeds on the Principles of *Galileo*, and ſtrenuouſly aſſerts the Flight of all Bullets to be in the Curve of a Parabola; undertaking to anſwer all Objections, that could be urged to the contrary. And in the Year 1683, Monſieur *Blondel* publiſhed at *Paris*, *L'Art de jetter les Bombes*, where the Doctrine of *Galileo* is likewiſe applied to the Motion of Shells and Bullets of all kinds, and the Variations of this Doctrine which can ariſe from the Reſiſtance of the Air, are particularly mentioned; and after a long Diſcuſſion the Author concludes, that they will be ſo very minute as ſcarcely to affect the Accuracy of his Concluſions *(a)*. Alſo, the ſame Subject is treated of in our Philoſophical Tranſactions *(b)* by Dr. *Hally*, who, ſwayed by the Conſideration of the very great Diſproportion between the Denſity of Bullets and of the Air, thinks it reaſonable to believe, that the Oppoſition of the Air to large Metal Shot is ſcarcely diſcernable, although in ſmall and light Shot he acknowledges, that it ought and muſt be accounted for.

In conſequence then of theſe Opinions about the inconſiderable Effects of the Air's Re-

(a) Vid. Page 345. of the firſt Quarto Edition at the Bottom, alſo Page 355. and following.

(b) Vid. No 216. p. 68.

ſiſtance

ſiſtance on heavy Shot, and the Demonſtrations of *Galileo*, that all Projectiles moved in the Curve of a Parabola, if they were not diſturbed by that Reſiſtance, it is now an Opinion generally advanced by the Writers on the Theory of Gunnery, that the Flight of Shot and Shells is nearly in the Curve of a Parabola; for the Truth of which, we may appeal to the profeſſed Authors on this Subject, who have wrote within the laſt forty Years.

But though this Hypotheſis went ſmoothly on with thoſe, who contented themſelves with Speculation only; yet *Anderſon*, who made a great Number of Trials, found it impoſſible to ſupport it without ſome new Modification. For though it does not appear, that he ever examined the comparative Ranges of either Cannon or Muſquet Shot when fired with their uſual Velocities; yet his Experiments on the Ranges of Shells thrown with ſmall Velocities (in reſpect of thoſe laſt mentioned) convinced him, that their whole Track was not Parabolical, as appears by his Treatiſe, intituled, *To hit a Mark*, publiſhed in the Year 1690. But inſtead of making the proper Inferences from hence, and diſcovering the Reſiſtance of the Air to be of conſiderable Efficacy, he from his great Attachment to his firſt Opinions framed

a new Hypotheſis, which was, that the Shell or Bullet at its firſt Diſcharge, flew a certain Diſtance in a right Line, from the End of which Line only it began to bend into a Parabola. And this right Line which he calls the Line of the Impulſe of the Fire, he ſuppoſes to be the ſame in all Elevations. By this Hypotheſis (though an indefenſible one) it was always in his Power, by aſſigning a proper Magnitude to this Line of Impulſe, to reconcile any two Shot made at different Angles, however oppoſite they might prove to the common Principles. But even this new-modelled Theory, was not, I believe, confirmed by his following Experiments; for he has no where ventured to give us Experiments of three Ranges made at three different Elevations, with the ſame Quantity of Powder; as finding, I preſume, that though by this Scheme he could reconcile two jarring Ranges, the Irregularities of three were inſurmountable. And, if ſuch Inequalities were produced by the Reſiſtance of the Air in the Motion of a Shell, impelled from a Mortar by an inconſiderable Quantity of Powder, what may not the Action of the Air be ſuppoſed to effect in the Motions of Bullets, which being impelled by a full Charge of Powder through a much longer Cylinder, move perhaps three or four Times

as faſt, and conſequently undergo near fifty Times the Reſiſtance, as will be more particularly evinced hereafter.

THAT the Reſiſtance of the Air, which acts with ſuch prodigious Power on all ſwift Bodies, ſhould be entirely unattended to by the Practitioners in Gunnery, is not the only remarkable Circumſtance which occurs in this Enquiry; for after the Publication of Sir *Iſaac Newton's Philoſophiæ Naturalis Principia Mathematica*, it might have been expected, that all Mathematicians ſhould have been convinced of its Energy, ſince in that immortal Work the Law and Quantity of this Reſiſtance to ſlow Motions is determined, and confirmed by many Experiments. Indeed the ſame Law when extended to ſwift Motions will be defective, and will exhibit the Reſiſtance greatly ſhort of what it really comes out by Experiment, (of which Sir *Iſaac Newton* himſelf has given us *(a)* warning;) yet even upon his Principles it would appear, that the Action of the Air on Bullets is by far too conſiderable to be neglected. But notwithſtanding this obvious Proof of the Neceſſity of conſidering the Action of the Air on military Projectiles, I can recollect but one Inſtance, where any Computa-

(a) Vid. Phil. Nat. Prin. Math. p. 351. l. 17.

tions founded on Sir *Iſaac Newton*'s Doctrine, have been applied to theſe Motions (*a*).

To ſum up now at once all we here intend to obſerve on this Head. It appears, that the modern Writers on the Art of Gunnery have been very much deceived, in ſuppoſing the Reſiſtance of the Air to be inconſiderable, and thence aſſerting, that the Track of Shot and Shells of all kinds is nearly in the Curve of a Parabola. That by this Means it has happened, that all their Determinations about the Flight of Shot diſcharged with conſiderable Degrees of Celerity are extreamly erroneous, and conſequently that the preſent Theory of Gunnery in this its moſt important Branch is uſeleſs and fallacious.

Now to obviate in ſome degree theſe Imperfections in this Art, we have undertaken in the ſecond Chapter of the enſuing Treatiſe, not only to confirm what we have here aſſerted relating to the Falſity of the parabolic Motion of theſe Projectiles, but likewiſe to aſcertain the actual Degree of Reſiſtance, which every Shot undergoes according to the Velocity with which it moves; whence, as the

(*b*) Vid comm. Acad. Petrop. Tom. 2. p. 338. 339.

Ve-

Velocity with which the Bullet iſſues from the Piece is eaſily known by the Principles delivered in the firſt Chapter, the Delineation of the Track paſſed through by the Bullet hereby becomes a geometrical Problem, which indeed in its utmoſt extent is of a very complicate and operoſe Kind; but in the Inſtances which are moſt frequent in practice, it admits of ſome very eaſy Approximations, which enable us readily to compare the actual Ranges of Bullets with the Reſult of this Theory.

And though ſuch as examine the following Treatiſe with Attention, will not, I believe, entertain many Doubts of the Certainty of the Determinations therein contained; yet it might have been expected perhaps, that the Accuracy of thoſe Principles ſhould have been ſtill more irrefragably eſtabliſhed by Experiments on the real Ranges of Pieces, compared with Computations founded on this Theory. And, indeed, I did once intend to have added a Chapter with this view; but two Reaſons have diverted me from this Deſign. The firſt was, the Difficulty I found in aſcertaining the extended Ranges; a Difficulty which none but thoſe who ſhall attempt Experiments of the ſame Kind, can be judges of. The ſecond Reaſon was an Irregularity which in-

intervened in theſe Ranges, and which rendered all my Endeavours fruitleſs; for the ſame Piece at the ſame Elevation would convey the Bullet to very diſtant Places, ſo that no two Trials agreed with each other, as I have more particularly recited in the 7th Propoſition of the 2d Chapter.

But notwithſtanding theſe Difficulties, which have hindered me from inſerting in the following Treatiſe ſuch Experiments on the Ranges of Shot, as might corroborate the Theory of Reſiſtance there delivered, I have yet reſolved to purſue this Subject; and I flatter myſelf, that I have invented a Method of preventing the laſt-mentioned Inequality from taking Place, which unleſs it can be done, it is ſufficiently obvious, how fruitleſs all Experiments of this kind muſt prove. The Reſult of my future Trials on this Head, I intend for a ſecond Part to this Treatiſe, in which, beſides theſe Experiments on the Track deſcribed by the Flight of Bullets, and the neceſſary geometrical Determinations with which they muſt be compared, I propoſe to inſert many other Experiments, which though of a miſcellaneous Nature, are yet all of them connected in ſome degree with the Theory or Practice of Gunnery. I ſhall alſo annex to

this

this ſecond Part many Maxims and practical Precepts, which will ariſe from the preceding Principles, and will, I hope, be of ſome Conſequence in the future Management of Artillery. A conſiderable Part of this ſecond Work I have already by me, as likewiſe an Apparatus purpoſely intended for compleating it. But thoſe Experiments which are yet wanting, will require great Leiſure and a proper Seaſon to execute.

As the following Sheets, beſides the Determination of the Quantity of the Air's Reſiſtance, do likewiſe contain the Theory of the Force and Action of Powder; it may perhaps be expected, that I ſhould give ſome Account of what preceding Authors have advanced on this Subject. But all, I have ever met with on this Head, hath been ſo vague and indiſtinct, that it is often difficult to determine the true Meaning of the Writer. The moſt intelligible Hypotheſis on this Head, and what ſeems indeed to have been the Original of all the others, is that of Monſieur *de la Hire*.

In the Hiſtory of the *French* Academy for the Year 1702, Monſieur *de la Hire* has ſuppoſed, that the Force of Powder may be owing to the increaſed Elaſticity of the Air contained

tained in, and between the Grains, in conſequence of the Heat and Fire produced at the Time of the Exploſion. Now, if this Air, to whoſe augmented Spring the Violence of Gunpowder is imputed, be in its natural State, at the Time when the Powder is fired (and ſurely what is in the Intervals of the Grains muſt be allowed to be ſo) the greateſt Addition its Elaſticity could acquire from the Flame of the Exploſion, would not amount to five times its uſual Quantity, as we ſhall more particularly evince hereafter (*a*); that is, it would not ſuffice for the two hundredth Part of the Effort, which we have found to be exerted by fired Powder.

HOWEVER, this Hypotheſis hath given riſe to many Diſſertations and Treatiſes in a neighbouring Nation, and one Author in particular conceives he has made a very reaſonable Poſtulate, in ſuppoſing the Elaſticity of the Air, when heated by the Exploſion of the Powder, to be only an hundred Times greater than when it is heated to the Degree of boiling Water. But as I think I have ſhewn the Impoſſibility of accounting for the actual Force of Power on theſe Principles, I will not detain the Reader any longer with a particular

(*a*) Vid. Prop. V. Cap. 1. *of the following Treatiſe.*

Re

Recital of the Speculations of theſe different Writers; eſpecially, as I flatter myſelf, that I have eſtabliſhed that Theory of the Force of Powder, which is contained in the following Sheets, by ſuch deciſive Experiments, as will render a formal Confutation of any other Opinion unneceſſary.

ERRATA.

PAGE 28. line 3. *for* L M *read* L N.
Pag 32. lin. 6. *Put a full Point after* Velocity, *and traſe that after* indeed.
Pag. 33. lin. 3 *and* 4, *for* ſenſibly, *read* ſenſible.
Pag. 36. lin. *the laſt*, *for* D 2 *read* D.
Pag. 42. lin. 8. *after* Length *add* and Weight.
Pag. 53. lin. 2. *for* two *read* too.
Pag. 62. lin. 33. *for* of *read* for. *Alſo* lin. 35. *for* clear *read* clean.
Pag. 63. lin. 20. *for* by *read* from.
Pag. 72. lin. 5. *eraſe* it.
Pag. 93. lin. 19. *for* rotary *read* rotatory.

Rational of the Speculations of thole different Writers; especially, as I have myself, that I have established that Theory of the Force of Oyster, which is contained in the following Sheets, by such decisive Experiments, as will render a formal Confutation of any other Opinion unnecessary.

CHAP. I.

Of the Force of Gunpowder.

PROPOSITION I.

Gunpowder, fired either in a Vacuum *or in Air, produces by its Exploſion a permanent elaſtic Fluid.*

IF a red hot Iron be included in a Receiver, and the Receiver be exhauſted, and Gunpowder be then let fall on the Iron, the Powder will take Fire, and the mercurial Gage will ſuddenly deſcend upon the Exploſion; and, tho' it immediately aſcends again, yet it will never riſe to the Height it firſt ſtood at, but will continue depreſſed by a Space proportioned to the Quantity of Gunpowder, which was let fall on the Iron. This is a well known Experiment, and is circumſtantially deſcribed by Mr. *Hauksbee*, in the *Philoſophical Tranſactions*, No. 295; in which Place he tells us, that he by this means (firing ſmall Quantities at a Time) reduced the Gage from

29½ Inches to 12¾. Now this Experiment, which has been often repeated, proves the Propoſition with reſpect to the Production of a permanent elaſtic Fluid in a *Vacuum*; for the Deſcent of the Gage could only be effected by the Preſſure of ſome new generated Fluid in the Receiver, ballancing in Part the Preſſure of the external Air. That this Fluid or ſome Part of it at leaſt was permanent, appears from what Mr. *Hauksbee* relates in the ſame Place, that tho' the Quickſilver aſcended after the Operation, yet it next Day had aſcended no higher than to 22½, at which Place it ſeemed to continue fix'd. And, that this Fluid is elaſtic, is proved from the Deſcent of the mercurial Gage; ſince the Quantity of Matter contained in this Fluid, could not by its Gravity alone have ſunk the Quickſilver by the leaſt ſenſible Quantity; alſo from its extending itſelf through any Space however great; the Experiment ſucceeding in either a large or ſmall Receiver, only the larger the Receiver the leſs will be the Deſcent of the mercurial Gage to the ſame Quantity of Powder; the Preſſure of the generated Fluid diminiſhing as its Denſity diminiſhes.

The ſame Production likewiſe takes Place, when Gunpowder is fired in the Air; * for if a ſmall Quantity of Powder be placed in the upper Part of a glaſs Tube, and the lower Part of the Tube be immerged in Water, and the Water be made to riſe ſo near the Top, that only a ſmall Portion of Air is left in that Part where the Gunpowder is placed; if in this Situation the Communication of the upper Part of the Tube with the external Air be cloſed, and the Gunpowder be fired (which may eaſily be done by a Burning-glaſs) the Water will in this Experiment deſcend on the Exploſion, as the Quickſilver did in the laſt, and will always

* Vid. *Hauksbee* Phyſ. Mechan. Exper. Page 81.

continue

continue depreſſed below the Place, at which it ſtood before the Exploſion, and the Quantity of this Depreſſion will be greater, if the Quantity of Powder be increaſed, or the Diameter of the Tube be diminiſhed. From whence it is proved, that as well in Air as in a *Vacuum*, the Exploſion of fired Powder produces a permanent elaſtic Fluid.

SCHOLIUM.

It has been known ever ſince the Time of Mr. *Boyle*, that many Subſtances, in Fermentation and other chymical Operations, produce elaſtic Fluids analogous in ſome of their Effects to the common Air. It is likewiſe known, that other Mixtures will in many Caſes abſorb a Part of the Air contiguous to them; in particular, it is obſerved, that all burning Bodies and all ſulphureous Fumes deſtroy great Quantities of Air, either by abſorbing it into their own Subſtance, or at leaſt by depriving it of its Elaſticity. This Creation and Conſumption of Air in chymical Proceſſes has been lately moſt diligently and ſucceſsfully examined by the Reverend Mr. *Hales* in his *Vegetable Statics*. And on theſe Principles it follows, that in the laſt Experiment the ſulphureous Fumes ariſing from the burning of the Charcoal and Brimſtone contained in the Powder muſt ſoon abſorb ſome of the Air, in which the Powder is fired; for which Reaſon it is neceſſary, that the Bulk of the Air, which the Powder is placed in, before it is fired, ſhould bear as ſmall a Proportion as poſſible to the Quantity of Powder, ſo that the Succeſs of the Experiment may not be diſturbed by the abſorbed Air, approaching to an Equality with the generated Fluid.

There is beſides another Reaſon, that, when Powder is fired in the Manner of the laſt Experiment, the Bulk of the Air, in which it is placed, ſhould be as little as poſſible; which is, that the Fire at the inſtant of

the Explosion will greatly augment the Elasticity of that Air, and the Pressure arising from this increased Elasticity, being added to the Force of the generated Fluid, will endanger the bursting of the Tube.

PROP. II.

To explain more particularly the Circumstances attending the Explosion of Gunpowder, either in a Vacuum *or in Air, when fired in the Manner described in the Experiments of the last Proposition.*

WHEN any considerable Quantity of Gunpowder is fired in an exhausted Receiver, by being let fall on a red hot Iron; the mercurial Gage instantly descends upon the Explosion, and as suddenly ascends again; and after a few Vibrations, none of which except the first are of any great Extent, it seemingly fixes at a Place lower than where it stood before the Explosion, and this stationary Point is, what we have always attended to in our Experiments. But even, when the Gage has acquired this Point of apparent Repose, it still continues rising for a considerable Time, altho' by such imperceptible Degrees, that it can only be discovered by comparing together its Place at distant Intervals; however, it will not always continue to ascend, but will rise slower and slower, till at last it will be absolutely fixed at a Point lower than where the Mercury stood before the Explosion.

The same Circumstances nearly happen, when Powder is fired in the upper Part of an unexhausted Tube, whose lower Part is immerged in Water.

Now these Appearances all arise from the different Modifications, which the Fluid produced from the Explosion undergoes. The first sudden Descent of the Mercury is effected by the Action of that Fluid, while in the Form of Flame. When the Flame is extinguished,

extinguiſhed, and conſequently the Heat of the Fluid is diminiſhed, its Elaſticity is likewiſe diminiſhed; and this being effected in a very ſhort Time, occaſions the ſudden Riſe of the Mercury after the firſt Deſcent. When the Fluid is reduced to the Temperature of the containing Receiver, its Elaſticity is then more fixed and invariable; and this muſt uſually happen by the Time the Mercurial Gage firſt appears to be ſtationary. The ſubſequent ſlow Aſcent of the Mercury is partly owing to the Decreaſe of the Heat of the Receiver, occaſioned by the cooling of the hot Iron contained in it; but much more to the Action of the ſulphureous Fumes of the Brimſtone and Charcoal, which abſorb a Part of the generated Fluid, and thereby diminiſh its Preſſure on the Gage.

SCHOLIUM.

In the following Propoſitions we ſhall irrefragably demonſtrate, that the Force of fired Gunpowder is nothing more than the Preſſure of the Fluid, which is generated in the preceeding Experiments; and that this Fluid in its Action obſerves the ſame Laws with other elaſtic Fluids, particularly the Air; ſo that whatever Power is produced by the firing of a given Quantity of Gunpowder, the ſame would be exerted by ſubſtituting in its ſtead a Quantity of Air equal to the Fluid generated in the Exploſion, provided that Air be included in the ſame Space, and be heated to the ſame Degree, as the other Fluid is, at the Inſtant of its firing. Mr. *Hales* has concluded, that the Weight of the factitious elaſtic Fluids produced from chymical Proceſſes is the ſame with that of common Air; he having tried that produced from Tartar with great Exactneſs. He has found too, that they expand with Heat, and contract with Cold, and that with the ſame Preſſure they are condenſed in the

ſame Degree with common Air. And that, when they are cleared of their ſulphureous Fumes, which is done by making them paſs through Water, they will then continue for many Months, nay Years, without loſing any conſiderable Part of their Elaſticity. And from theſe and other Circumſtances he doubts not to aſſert, that theſe Fluids are true permanent Air. Now if this be ſuppoſed of all, or any of the elaſtic Fluids produced by Diſtillation, Burning, *&c.* it muſt be preferably allowed to be true of that Fluid, which is generated in the Exploſion of Gunpowder; ſince it is from Saltpetre alone, that this Fluid ſeems to be derived (for neither the Brimſtone nor the Charcoal yield it, when fired by themſelves) and Saltpetre is known to be a Subſtance imbibed from the Air by Earth; for the ſame Parcel of Earth by being properly expoſed to the Air will furniſh Saltpetre over and over again for ever. However, tho' it be highly reaſonable to ſuppoſe, that the elaſtic Fluid ariſing from the firing of Powder, is genuine and permanent Air; yet the Truth or Falſhood of this Suppoſition no ways affects the Certainty of our Concluſions. It is ſufficient for our Purpoſe, that it is an elaſtic Fluid; whether it be Air or another Compoſition our Reaſoning will be ſtill the ſame, ſince it is by Experiments on this Fluid itſelf, and not by obſcure Speculations on its Nature and Qualities, that our future Deductions relating to its Force and Action are confirmed.

PROP. III.

The Elaſticity or Preſſure of the Fluid produced by the firing of Gunpowder, is cæteris paribus *directly as its Denſity.*

THIS follows from hence, that, if in the ſame Receiver a double Quantity of Powder be let fall, the Mercury will ſubſide twice as much as in the firing of a ſingle Quantity. For the Vapour produced from the double Quantity being contained in the ſame Receiver will be of double the Denſity of that produced from the ſingle Quantity; whence the Elaſticity or Preſſure eſtimated by the Deſcent of the Mercury being likewiſe double, the Preſſure is directly as its Denſity. Alſo the Deſcents of the Mercury, when equal Quantities of Powder are fired in different Receivers, are reciprocally as the Capacities of thoſe Receivers; and conſequently as the Denſity of the produced Fluid in each.

But as in the uſual Method of trying this Experiment, the Quantities of Powder are ſo very ſmall, that it is difficult to aſcertain theſe Proportions to a requiſite Degree of Exactneſs, I took a large Receiver containing about 520 Inches, and letting fall at once on the red hot Iron, 1 Dram or the $\frac{1}{16}$ of an Ounce Avoirdupoiſe of Powder (the Receiver being firſt nearly exhauſted,) the Mercury after the Exploſion was ſubſided 2 Inches exactly, and all the Powder had taken Fire. Then heating the Iron a ſecond Time, and exhauſting it as before, 2 Drams were let down at once, which ſunk the Mercury $3\frac{3}{4}$ and a ſmall Part of the Powder had fallen beſide the Iron, which (the Bottom of the Receiver being wet) did not fire, and the Quantity, that thus eſcaped, did appear to be nearly ſufficient, had it fallen on the

Iron, to have ſunk the Mercury ¼ Part of an Inch more; in which Caſe the two Deſcents, *viz.* 2 Inches, and 4 Inches, would have been accurately in the Proportion of the reſpective Quantities of Powder; from which Proportion, as it was, they very little varied.

Hence then it appears, that the Elaſticity of the Vapour produced by Gunpowder in its Exploſion, is directly as its Denſity.

PROP. IV.

To determine the Elaſticity, and Quantity of this elaſtic Fluid, produced from the Exploſion of a given Quantity of Gunpowder.

AS different Kinds of Gunpowder produce different Quantities of this Fluid in Proportion to their different Degrees of Goodneſs, before any definite Determination of this kind can take Place it is neceſſary to aſcertain the particular Species of Powder that is propoſed to be uſed; and therefore I ſhall in every Examination and Poſition relating to this Subject ſuppoſe the Powder in Queſtion to be of the ſame ſort with what is made for the Uſe of the Government; that being by Contract to conſiſt of a known and invariable Proportion of Materials; and is therefore much properer for a Standard, than what is compounded according to the arbitrary Fancy of the Artiſt.

This being ſettled, we muſt further premiſe theſe two Principles, which we have already mentioned in the Scholium to Prop II. the firſt, that the Elaſticity of this Fluid increaſes by Heat, and diminiſhes by Cold, in the ſame Manner as that of the Air; the ſecond, that the Denſity of this Fluid and conſequently its Weight is the ſame with the Weight of an equal Bulk of Air, having the ſame Elaſticity, and the ſame Temperature.

Now

Now from the Experiment recited in the laſt Propoſition, it appears that $\frac{1}{18}$ of an Ounce Avoirdupoiſe or about 27 Grains Troy, of Powder, ſunk the Gage on its Exploſion 2 Inches; and the Mercury in the Barometer ſtanding at near 30 Inches, $\frac{15}{19}$ of an Ounce Avoirdupoiſe, or 410 Grains Troy, would have filled the Receiver with a Fluid, whoſe Elaſticity would have been equal to the whole Preſſure of the Atmoſphere, or the ſame with the Elaſticity of the Air we breathe; and the Content of the Receiver being about 520 cubic Inches, it follows, that $\frac{15}{16}$ of an Ounce of Powder will produce 520 cubic Inches of a Fluid, poſſeſſing the ſame Degree of Elaſticity with common Air; whence an Ounce of Powder will produce near 575 cubic Inches of ſuch a Fluid.

But in order to aſcertain the Denſity of this Fluid we muſt conſider, what Part of its Elaſticity at the time of this Determination was owing to the Heat it received from the included hot Iron and the warm Receiver. Now the general Heat of the Receiver being manifeſtly leſs than that of boiling-Water, which is known to increaſe the Elaſticity of the Air, ſomewhat more than $\frac{1}{4}$ of its augmented Quantity, I collect from hence and other Circumſtances, that the Augmentation of Elaſticity ariſing from this Cauſe was about the $\frac{1}{5}$ of the whole, that is, if the Fluid ariſing from the Exploſion had been reduced to the Temperatue of the external Air, the Deſcent of the Mercurial Gage inſtead of 2 Inches would have been only $1\frac{3}{5}$ Inch; whence 575, reduced in the Proportion of 5 to 4, becomes 460, and this laſt Number repreſents the cubic Inches of an elaſtic Fluid equal in Denſity and Elaſticity with common Air, which are produced from the Exploſion of 1 Ounce Avoirdupoiſe of Gunpowder, the Weight of which Quantity of Fluid according to the uſual Eſtimation of the Weight of Air is 131 Grains;

whence

whence the Weight of this Fluid is $\frac{10}{437}$ or $\frac{1}{10}$ nearly of the Weight of the generating Powder.

If the Ratio of the Bulk of the Gunpowder to the Bulk of this Fluid be wanted, this will be determined by knowing, that 1 Ounce 1 Dram, or 17 Drams Avoirdupoiſe of Powder, fill 2 cubic Inches, if the Powder be well ſhook together; wherefore augmenting the Number laſt found in the Proportion of 16 to 17, the reſulting Term 488 $\frac{3}{4}$ is the Number of cubic Inches of an elaſtic Fluid, equal in Denſity with the Air produced from 2 cubic Inches of Powder; whence the Ratio of the reſpective Bulks of the Powder, and the Fluid produced from it, is in round Numbers, 1 to 244.

And farther, to confirm this Determination, I fired the Quantity of a Dram of Powder four times ſucceſſively, in an exhauſted Receiver by a Burning-glaſs; the Capacity of this Receiver was 470 cubic Inches. Theſe Experiments were more troubleſome than thoſe, in which it was fired by a hot Iron, becauſe it was ſometimes long before it would fire; in which Interval the Air would often inſinuate itſelf, and thereby diſturb the Meaſures of the Deſcent; and beſides, near $\frac{1}{4}$ Part of the Powder was uſually diſſipated unfired by the Blaſt: However, by collecting the Grains that were thus ſcattered, and weighing them, and increaſing the Deſcent by a proportional Quantity, the Subſiding of the Mercury, correſponding to one Dram of Powder, was the firſt time 2, 1 + Inches, the ſecond time 1, 8 — Inches, the third time 2, 1 —, and the fourth time 1, 8 5 Inches, or at a Medium 1, 9 6 Inches; and this, diminiſhed in the Ratio of 520 to 470, becomes 1, 77 for the Deſcent to a like Quantity in the firſt Receiver. Now the Deduction to be made on Account of the Heat of the Receiver was but little in theſe Experiments; for, by including a ſmall Thermometer, I found that the Fluid within the Re-

ceiver

ceiver was not hotter after the Blaſt than that of the Summer Air; whence, if the Deſcent 1,77 be reduced in the Ratio of 13 to 12, which is nearly that of the Elaſticity of hot Summer Air to temperate Air, it becomes 1,63 nearly, which differs little from 1 $\frac{2}{3}$, or 1,6; which is, what we found it to be in the preceding Experiment: Whence the Proportion between the reſpective Bulks of the Powder, and the Fluid produced from it, may be ſtill aſſumed to be that of 1 to 244.

And this Ratio agrees very well with the Experiment recited by Mr. *Hauksbee* in his *Phyſico-Mechanic Experiments*, p. 81; for he there found, that one Grain of Powder produced, when fired in the Air, a cubic Inch of elaſtic Fluid, which, ſuppoſing the Denſity of Powder to be what we have here aſſigned, gives the Ratio of their reſpective Bulks to be that of 1 to 232; a Difference, from what we have aſſigned above, that may eaſily ariſe from the Difference of the Powder only. Whence we may conclude, that the Preſence of a greater or leſs Quantity of Air does not affect the Production of this Fluid; ſince, by comparing Mr. *Hauksbee*'s Experiment with our own, it appears, that the ſame Quantity of this Fluid is generated in a *Vacuum* as in the Air.

If this Fluid, inſtead of expanding, when the Powder was fired, had been confined in the ſame Space, which the Powder filled before the Exploſion; then (its Elaſticity having been ſhewn to be as its Denſity) it would have had, in that confined State, a Degree of Elaſticity 244 times greater than that of common Air, and this independent of the great Augmentation this Elaſticity would receive from the Action of the Fire in that inſtant.

Hence then we are certain, that any Quantity of Powder fired in any confined Space, which it adequately fills, exerts at the Inſtant of its Exploſion

againſt

againſt the Sides of the Veſſel containing it, and the Bodies it impells before it, a Force at leaſt 244 times greater than the Elaſticity of common Air, or, which is the ſame thing, than the Preſſure of the Atmoſphere; and this without conſidering the great Addition which this Force will receive from the violent Degree of Heat, with which it is endued at that Time; the Quantity of which Augmentation is the next Head of our Enquiry.

PROP. V.

To determine how much the Elaſticity of the Air is augmented, when heated to the extremeſt Heat of red-hot Iron.

TO fix this Point, I took a Piece of a Muſket-barrel, about ſix Inches in Length, and order'd one End to be cloſed up entirely, but the other End was drawn out conically, and finiſhed in an Aperture of about $\frac{1}{8}$ of an Inch in Diameter. This Tube, thus fitted, was heated to the Extremity of a red Heat in a Smith's Forge, and was then immerged with its Aperture downwards in a Bucket of Water, and kept there, till it was cool; after which it was taken out carefully, and the Water, which had enter'd it in cooling, was exactly weigh'd. The Weight of the Water thus taken in at three different Trials was 610 Grains, 595 Grains, and 600 Grains reſpectively. The Content of the whole Cavity of the Tube was 796 Grains of Water; whence the Spaces remaining unfilled in theſe three Experiments were equal in Bulk to 186, 201, 196 Grains of Water reſpectively; and theſe Spaces did doubtleſs contain all the Air, which, when the Tube was red-hot, did extend through its whole Concavity; conſequently the Elaſticity of the Air, when heated to the extreme Heat of red-hot Iron,

was

was to the Elaſticity of the ſame Air, when reduced to the Temperature of the ambient Atmoſphere, as the whole Capacity of the Tube to the reſpective Spaces taken up by the cooled Air, that is, as 796 to 186, 201, 196, or taking the Medium of theſe three Trials, as 796 to 194 $\frac{1}{3}$.

The Heat given to the Tube each Time was the Beginning of what Workmen call a white Heat; and to prevent the ruſhing in of the aqueous Vapour at the Immerſion, which will otherwiſe drive out great Part of the Air, and render the Experiment fallacious, I had an Iron Wire filed tapering, ſo as to fit the Aperture of the Tube, and with this I always ſtopt it up, before it was taken from the Fire, letting it remain in till the whole was cool, when removing it, the due Quantity of Water would enter.

PROP. VI.

To determine how much that Elaſticity of the Fluid produced by the firing of Gunpowder, which we have above aſſigned, is augmented by the Heat, it has at the Time of its Exploſion.

AS Air and this Fluid appear to be equally affected by Heat and Cold, and conſequently have their Elaſticities equally augmented by the Addition of equal Degrees of Heat to each, if we ſuppoſe the Heat, with which the Flame of fired Powder is endued, to be the ſame as that of the extreme Heat of red-hot Iron, then the Elaſticity of the generated Fluid will be greater at the Time of Exploſion, when it is in the Form of Flame, than afterwards, when it is reduced to the Temperature of the ambient Air, in the Ratio of 796 to 194 $\frac{1}{3}$ nearly, that is, in the Ratio of the Elaſticities of common Air, under ſimilar Circumſtances, aſcertained in the laſt Propoſition.

Now

Now that the Heat of Powder, when fired in any conſiderable Quantity, is not leſs than that of red-hot Iron, ſeems ſufficiently evident from the Appearance of the Flame, and the known Properties of ſome of its Materials; for the Fire produced by the Exploſion is certainly as active as any common Fire; and it is well known, that all Fires will communicate a red-hot Heat to Iron, provided the Bulk of the Iron be ſufficiently ſmall, when compared with the Quantity of the Fire.

This being ſuppoſed then, that the Flame of fired Gunpowder is not leſs hot than red-hot Iron, and the Elaſticity of the Air, and conſequently of the Fluid generated by the Exploſion, being augmented by the Extremity of this Heat in the Ratio of $194\frac{1}{3}$ to 796, as has been ſhewn in the laſt Propoſition, it follows, that if 244 be augmented in this Ratio, the reſulting Number, which is $999\frac{1}{3}$, will determine how many times the Elaſticity of the Flame of fired Powder exceeds the Elaſticity of common Air, ſuppoſing it to be confined in the ſame Space which the Powder filled, before it was fired. For ſince we have ſhewn, in the 3d Propoſition, that the elaſtic Fluid produced from the firing a Quantity of Powder would, if confined in the ſame Space which the Powder took up before its Exploſion, exert an Elaſticity 244 times greater than the Elaſticity of common Air, ſuppoſing the Temperature of that Fluid and of the Air to be the ſame; it is plain from hence, that, when 244 is increaſed in the Ratio, in which the Elaſticity of this Fluid is greater at the Time of the Exploſion than afterwards, the reſulting Number will aſcertain, how many times the Elaſticity of this inflamed Fluid, at the Inſtant of its Exploſion, and before it has dilated itſelf, exceeds the Elaſticity of common Air.

Hence

Hence then the abſolute Quantity of the Preſſure exerted by Gunpowder at the Moment of its Exploſion may be aſſigned; for, ſince the Fluid, then generated, has an Elaſticity 999, or in round Numbers, 1000 times greater than common Air; and ſince common Air by its Elaſticity exerts a Preſſure on any given Surface equal to the Weight of the incumbent Atmoſphere, with which it is *in Equilibrio*, the Preſſure exerted by fired Powder, before it has dilated itſelf, is one thouſand times greater than the Preſſure of the Atmoſphere, and conſequently the Quantity of this Force, on a Surface of an Inch ſquare, amounts to above 6 Tun Weight, which Force however diminiſhes, as the Fluid dilates itſelf, according to what has been ſhewn in the third Propoſition.

SCHOLIUM.

Tho' we have here ſuppoſed, that the Heat of Gunpowder, when fired in any conſiderable Quantity, is the ſame with Iron heated to the Extremity of a red Heat, or to the Beginning of a white Heat, (which Determination we ſhall hereafter confirm by many Experiments) yet it cannot be doubted, but that the Fire produced in the Exploſion is ſomewhat varied (like all other Fires) by a greater or leſs Quantity of Fuel; and it may be preſumed, that, according to the Quantity of Powder fired together, the Flame may have all the different Degrees from that of a languid red Heat to the Heat ſufficient for the Vitrification of Metals; but as the Quantity of Powder requiſite for the Production of this laſt mentioned Heat, is certainly greater than what is ever fired together for any military Purpoſe, we ſhall find, by our future Experiments, that we ſhall not be far from our Scope, if we ſuppoſe the Heat of ſuch Quantities

as

as come more frequently in Uſe to be, when fired, nearly the ſame with the ſtrongeſt Heat of red-hot Iron; allowing a gradual Augmentation to this Heat in larger Quantities, and diminiſhing it when the Quantities are very ſmall.

PROP. VII.

Given the Dimenſions of any Piece of Artillery, the Denſity of its Ball, and the Quantity of its Charge, to determine the Velocity which the Ball will acquire from the Exploſion, ſuppoſing the Elaſticity of the Powder at the firſt Inſtant of its firing to be given.

IN the Solution of this Problem, we ſhall aſſume the two following Principles:

I. That the Action of the Powder on the Bullet ceaſes, as ſoon as the Bullet is got out of the Piece.

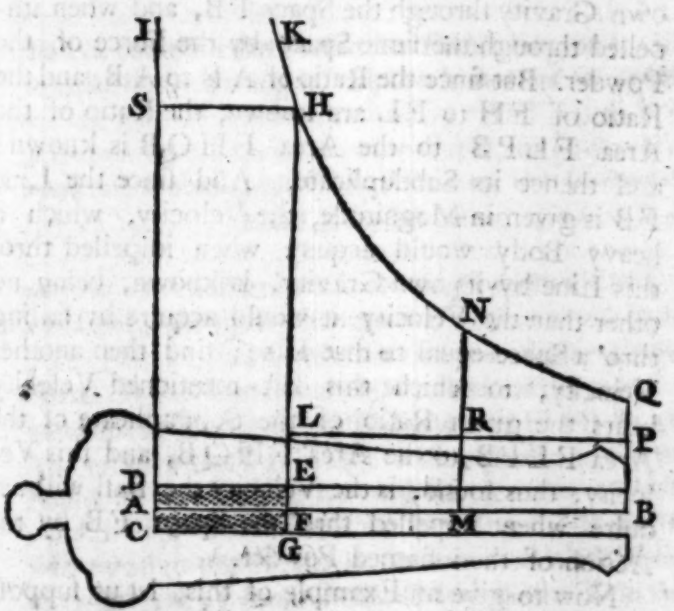
II. That all the Powder of the Charge is fired, and converted into an elaſtic Fluid, before the Bullet is ſenſibly moved from its Place.

Theſe Poſtulates we ſhall demonſtrate in an annexed *Scholium*; and they being ſuppoſed, the Propoſition itſelf is thus determined:

Let A B repreſent the Axis of any Piece of Artillery, A the Breech, and B the Muzzle; D C the Diameter of its Bore, and D E G C a Part of its Cavity filled with Powder. Suppoſe the Ball, that is to be impelled, to lie with its hinder Surface at the Line G E, then the Preſſure exerted at the Exploſion, on the Circle of which G E is the Diameter, or, which is the ſame thing, the Preſſure exerted in the Direction F B, on the Surface of the Ball, is eaſily known

from

from the known Dimenſions of that Circle; draw any Line F H perpendicular to F B, and A I parallel to F H, and through the Point H, to the Aſymptotes I A and A B, deſcribe the Hyperbola K H N Q; then if F H repreſents the Force impelling the Ball at the Point F, the Force impelling the Ball in any other Place as M will be repreſented by the Line M N, the Ordinate to the Hyperbola at that Point; for when the Fluid impelling the Body along, has dilated itſelf to M, its Denſity will be then to its original Denſity in the Space D E G C reciprocally as the Spaces, thro' which it is reſpectively extended; that is, as F A to M A, or as M N to E H; but we have ſhewn in the ſecond Propoſition, that the Elaſticity or impelling Force of this Fluid is directly as its Denſity; therefore, if F H repreſents that Force at the Point F, M N will repreſent the like Force at the Point M.



Since the abſolute Quantity of the Force impelling the Ball at the Point F is known, and the

 Weight

Weight of the Ball is likewiſe known; the Proportion between the Force, the Ball is impelled with, and its own Gravity is known. In this Proportion take FH to FL, and draw LP parallel to FB; then MN the Ordinate to the Hyperbola in any Point will be to its Part MR, cut off by the Line LP, as the impelling Force of the Powder in that Point M, to the Gravity of the Ball; and conſequently, the Line LP will determine a Line proportional to the uniform Force of Gravity in every Point; whilſt the Hyperbola HNQ determines in like Manner ſuch Ordinates, as are proportional to the impelling Force of the Powder in every Point; whence, by the 39th Propoſition of Lib. 1. of Sir *Iſaac Newton*'s *Phil. Nat. Prin. Math.* the Areas FLPB and FHQB are in the duplicate Proportion of the Velocities, which the Ball would acquire, when acted on by its own Gravity through the Space FB, and when impelled through the ſame Space by the Force of the Powder. But ſince the Ratio of AF to AB, and the Ratio of FH to FL are known, the Ratio of the Area FLPB to the Area FHQB is known; and thence its Subduplicate. And ſince the Line FB is given in Magnitude, the Velocity, which a heavy Body would acquire when impelled thro' this Line by its own Gravity, is known, being no other than the Velocity it would acquire by falling thro' a Space equal to that Line; find then another Velocity, to which this laſt mentioned Velocity bears the given Ratio of the Subduplicate of the Area FLPB, to the Area FHQB, and this Velocity, thus found, is the Velocity the Ball will acquire when impelled thro' the Space FB by the Action of the inflamed Powder.

Now to give an Example of this, let us ſuppoſe AB the Length of the Cylinder to be 45 Inches, its Diameter DC, or rather the Diameter of the Ball, to

be

be ¼ of an Inch; and AF the Extent of the Powder to be 2 ½ Inches; to determine the Velocity which will be communicated to a leaden Bullet by the Exploſion, ſuppoſing the Bullet laid at firſt with its Surface contiguous to the Powder.

By the Theory we have laid down in the laſt Propoſition, it appears, that at the firſt Inſtant of the Exploſion the Flame will exert, on the Bullet lying cloſe to it, a Force 1000 times greater than the Preſſure of the Atmoſphere; the Medium Preſſure of the Atmoſphere is eſteemed equal to that of a Column of Water 33 Feet high; whence Lead being to Water as 11,345 to 1, this Preſſure will be equal to that of a Column of Lead 34,9 Inches in Height, whence multiplying this by 1000, a Column of Lead 34900 Inches high would produce a Preſſure equal to what is exerted on the Ball by the Powder in the firſt Inſtant of the Exploſion, and the leaden Ball being ¼ of an Inch in Diameter, and conſequently equal to a Cylinder of Lead on the ſame Baſe ¼ an Inch in Height, the Preſſure at firſt acting on it will be equal to 34900x2 or 69800 times its Weight; whence FL to F H is as 1 to 69800: And FB to FA is as 45—2⅝ (or 42 ⅜) to 2 ½; that is, as 339 to 21, whence the Rectangle F L P B is to the Rectangle A F H S, as 339 to 21 × 69800; that is, as 1 to 4324—. And from the known Application of the Logarithms to the Menſuration of the Hyperbolic Spaces it follows, that the Rectangle A F H S is to the Area F H Q B as ,43429 &c. is to the tabular Logarithm of $\frac{AB}{AF}$; that is, of $\frac{160}{21}$, which is 1,2340579, whence the Ratio of the Rectangle F L P B to the Hyperbolic Area F H Q B is compounded of the Ratios of 1 to 4324—, and of ,43429 &c. to 1,2340579, which together make up the Ratio of 1 to 12263, the Subduplicate of which is the Ratio of 1 to 110,7; and in this Ratio is the Velocity, which the Bullet would acquire by Gravity, in falling through a Space equal to F B, to the Velocity

the Bullet will acquire from the Action of the Powder, impelling it through FB, but the Space FB being 42⅛ Inches, the Velocity a heavy Body will acquire in falling through ſuch a Space is known to be, what would carry it nearly at the Rate of 15,07 Feet in 1″ of Time; whence the Velocity, to which this has the Ratio of 1 to 110,7, is a Velocity, which would carry the Ball at the Rate of 15,07 × 110,7 Feet in 1″ of Time; that is, at the Rate of 1668 Feet in 1″ of Time. And this is the Velocity, which, according to the Theory, the Bullet in the preſent Circumſtances would acquire from the Action of the Powder, during the Time of its Dilatation.

And this being once computed for one Caſe is eaſily applied to any other; for, if the Cavity DE GC left behind the Bullet be only in Part filled with Powder, then the Line HF, and conſequently the *Area* FHQB will be diminiſhed in the Proportion of the whole Cavity to the Part filled; if the Diameter of the Bore be varied, the Lengths AB and AF remaining the ſame, then the Quantity of Powder and the Surface of the Bullet, which it acts on, will be varied in the duplicate Proportion of the Diameter; but the Weight of the Bullet will vary in the triplicate Proportion of the Diameter; wherefore the Line FH, which is directly as the abſolute impelling Force of the Powder, and reciprocally as the Gravity of the Bullet, will change in the reciprocal Proportion of the Diameter of the Bullet. If AF the Heighth of the Cavity left behind the Bullet be increaſed or diminiſhed, the Rectangle of the Hyperbola, and conſequently the *Area* correſponding to ordinates in any given Ratio, will be increaſed or diminiſhed in the ſame Proportion. From all which it follows, that the *Area* FHQB, which is in the duplicate Proportion of the Velocity of the impelled Body, will be directly as the Logarithm $\frac{AB}{AF}$, (where AB repreſents the Length of the Barrel, and AF the Lengh of the Cavity left behind the Bullet) alſo directly as the Part of that Cavity

Cavity filled with Powder, and inverſely as the Diameter of the Bore or rather of the Bullet, likewiſe directly as AF the Height of the Cavity left behind the Bullet. Conſequently the Velocity being computed above for a Bullet of a determined Diameter placed in a Piece of a given Length and impelled by a given Quantity of Powder, occupying a given Cavity behind that Bullet, it follows, that by means of theſe Ratios the Velocity of any other Bullet may be thence deduced, the neceſſary Circumſtances of its Poſition, Quantity of Powder, &c. being given. Where note, that in the Inſtance of this Propoſition we have ſuppoſed the Diameter of the Ball to be $\frac{1}{3}$ of an Inch: Whence the Diameter of the Bore will be ſomething more, and the Quantity of Powder contained in the Space D E G C will amount to exactly 12dw. a ſmall Wad of Tow included.

SCHOLIUM.

In this Propoſition we have taken for granted,

1ſt. That the Action of the Powder on the Bullet ceaſes, as ſoon as the Bullet is got out of the Piece.

2dly, That all the Powder of the Charge is fired, before the Bullet is ſenſibly moved from its Place.

Theſe Aſſumptions we are now to demonſtrate.

The firſt will, I preſume, appear manifeſt, when it is conſidered, how ſuddenly the Flame will extend itſelf on every ſide, by its own Elaſticity, when it is once got out of the Mouth of the Piece, for by this means its Force will then be diſſipated, and the Bullet will be no longer ſenſibly affected by it.

The ſecond Principle is indeed leſs obvious, being contrary to the general Opinion of almoſt all Writers on this Subject. But however it is not leſs certain. It might perhaps be ſufficient for the

Proof of this Poſition to obſerve the prodigious Compreſſion of the Flame in the Chamber of the Piece. Thoſe, who will attend to this Circumſtance and to the eaſy Paſſage of the Flame through the Intervals of the Grains, may ſoon ſatisfy themſelves, that no one Grain contained in that Chamber can continue for any Time uninflamed, when thus ſurrounded and violently preſſed by ſo active a Fire. However, not to rely on mere Speculation in a Point of ſo much Conſequence, I conſidered, that, if Part only of the Powder is fired, and that ſucceſſively, then by laying a greater Weight before the Charge (ſuppoſe 2 or 3 Bullets inſtead of one) a greater Quantity of Powder would neceſſarily be fired, ſince a heavier Weight would be a longer Time in paſſing through the Barrel. Whence it ſhould follow, that two or three Bullets would be impelled by a much greater Force than one only. But the contrary to this appears by Experiment; for firing one, two, and three Bullets laid contiguous to each other with the ſame Charge reſpectively, I have found (by a Method to be mentioned hereafter) that their Velocities were not much different from the Reciprocal of the Subduplicate of their Quantities of Matter, that is, if a given Charge would communicate to one Bullet a Velocity of 1700 Feet in 1″, the ſame Charge would communicate to two Bullets a Velocity from 1250 to 1300 Feet in 1″, and to three Bullets a Velocity from 1050 to 1110 Feet in one 1″. From hence it appears, that, whether the Piece be loaded with a greater or leſs Weight of Bullet, the Action of the Powder is nearly the ſame; ſince all Mathematicians know, that if Bodies containing different Quantities of Matter are ſucceſſively impelled thro' the ſame Space by the ſame Power, acting with a determined Force at each Point of that Space, then the Velocities given to thoſe different Bodies will be reciprocally in the ſubduplicate Ratio of

their

their Quantities of Matter. The Exceſs of the Velocities of the two and three Bullets above what they ſhould have been by this Rule, (which are that of 1200 and 980 Feet in 1″.) does doubtleſs ariſe from the Flame, which eſcaping by the Side of the firſt Bullet acts on the Surface of the ſecond and third.

Now this Exceſs has in many Experiments been imperceptible, and the Velocities have been reciprocally in the ſubduplicate Ratios of the Number of Bullets to ſufficient Exactneſs; and where this Error has been greater, it has never ariſen to an eighth Part of the whole; but if the common Opinion was true, that a ſmall Part only of the Powder fires at firſt, and other Parts of it ſucceſſively, as the Bullet paſſes through the Barrel, and that a conſiderable Part of it is often blown out of the Piece without firing at all; then the Velocity, which three Bullets received from the Exploſion, ought to have been much greater, than we have ever found it to be; ſince the Time of the Paſſage of three Bullets thro' the Barrel being nearly double the Time, in which one paſſes, it ſhould happen, according to this vulgar Suppoſition, that in a double Time a much greater Quantity of the Powder ſhould be fired, and conſequently a greater Force ſhould have been produced, than what acted on the ſingle Bullet only, contrary to all our Experiments.

But further, the Truth of the ſecond Poſtulate will be more fully evinced, when it ſhall appear, as it will hereafter, that the Rules founded on this Suppoſition aſcertain the Velocities of Bullets impelled by Powder, to the ſame Exactneſs, when they are acted on through a Barrel of 4 Inches in Length only, as when they are diſcharged from one of four Feet.†

With Reſpect to the Grains of Powder, which are often blown out unfired, and which are always

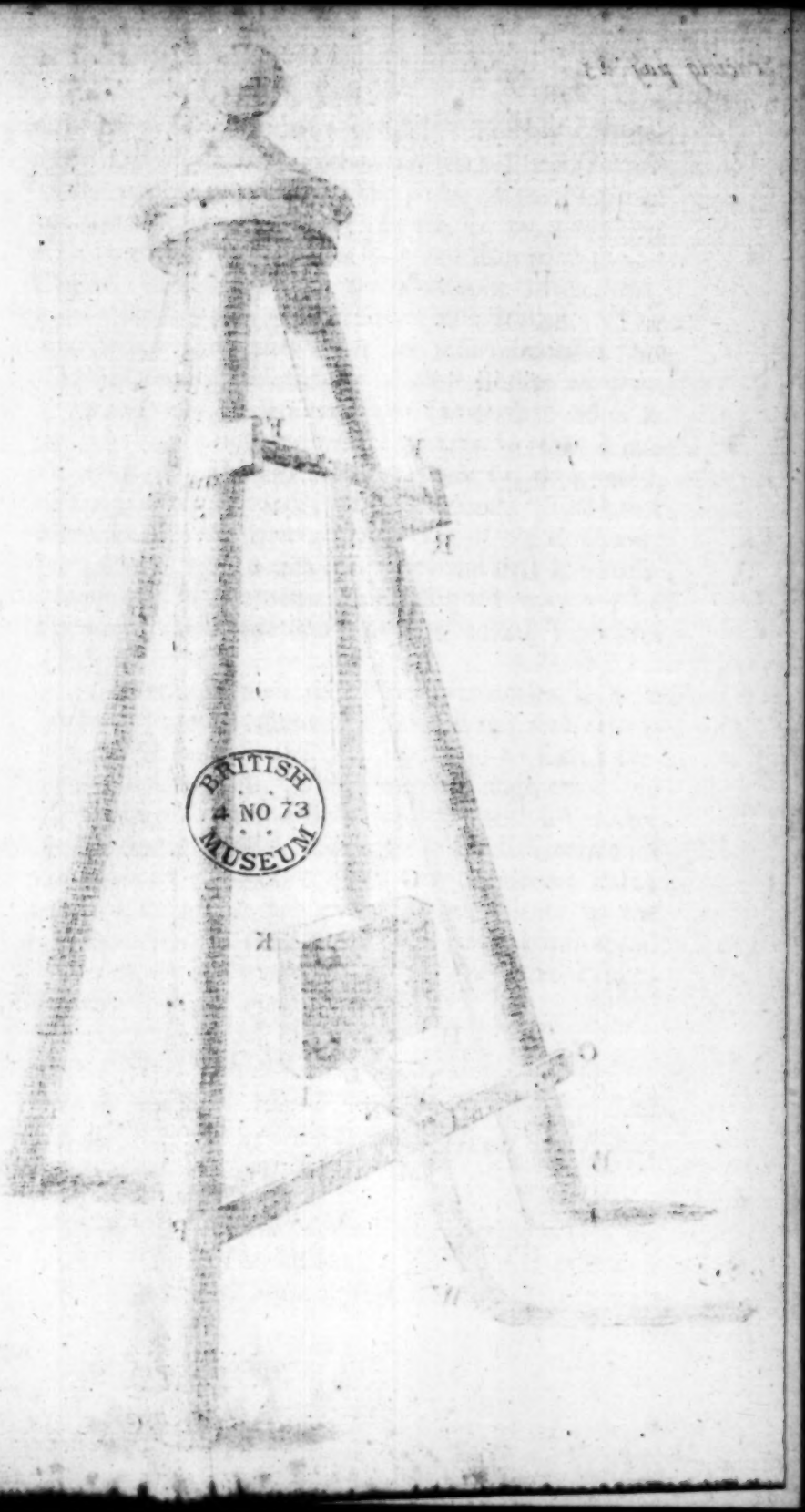
urged as a Proof of the gradual firing of the Charge, I believe *Diego Uffano*, a Perſon of great Experience in the Art of Gunnery, has given the true Reaſon for this Accident, which is, that ſome ſmall Part of the Charge is often not rammed up with the reſt, but is left in the Piece before the Wad, and is by this Means expelled by the Blaſt of Air before the Fire can reach it*; I muſt add, that in the Charging of Cannon and ſmall Arms, eſpecially after the firſt Time, this is ſcarcely to be avoided by any Method I have yet ſeen practiſed. Perhaps too, there may be ſome few Grains in the beſt Powder of ſo heterogeneous a Compoſition as to be leſs ſuſceptible of firing, which I think I have myſelf obſerved; theſe, tho' they are ſurrounded by the Flame, may be driven out unfired. However, be that as it may, the Truth of our Poſition cannot in general be queſtioned.

Having in this Propoſition ſhewn how the Velocity, which any Bullet acquires from the Force of Powder, may be computed upon the Principles of the Theory laid down in the preceeding Propoſitions of this Treatiſe, we will next ſhew, that the actual Velocities, with which Bullets of different Magnitudes are impelled from different Pieces, with different Quantities of Powder, are really the ſame with the Velocities aſſigned by theſe Computations, and conſequently, that this Theory of the Force of Powder, here delivered, does unqueſtionably aſcertain the true Action and Modification of this enormous Power.

But in order to compare the Velocities communicated to Bullets by the Exploſion with the Velocities reſulting from the Theory by Computation, it is neceſſary, that the actual Velocities, with which Bullets move, ſhould be capable of being diſcovered, which yet is impoſſible to be done by any

* Dialog. 20.

Methods



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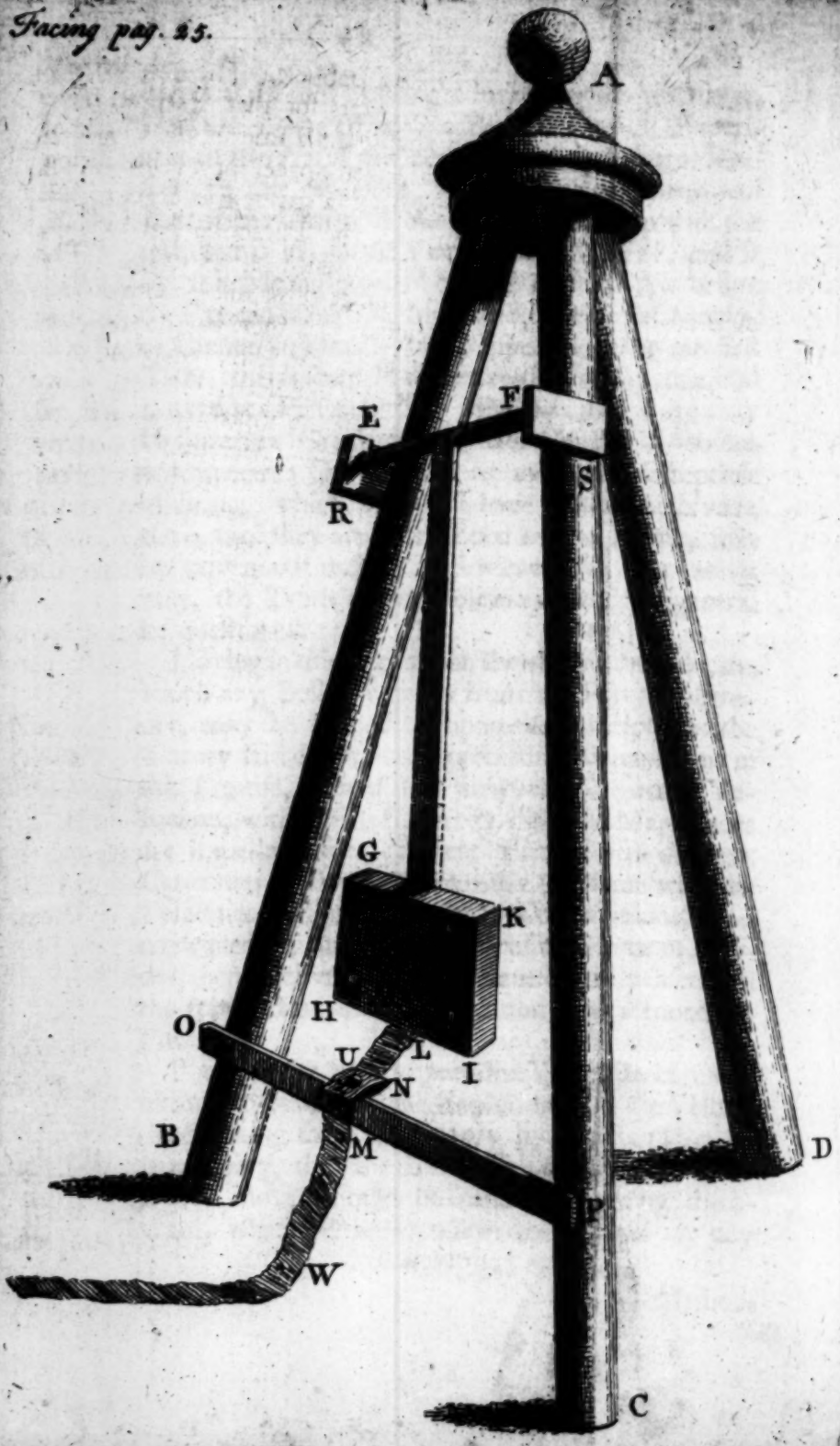

Methods hitherto made publick. The only Means hitherto practiſed by others for that Purpoſe have been either by obſerving the Time of the Flight of the Shot through a given Space, or by meaſuring the Range of the Shot at a given Elevation; and thence computing, on the Parabolic Hypotheſis, what Velocity would produce this Range. The firſt Method labours under this inſurmountable Difficulty, that the Velocities of theſe Bodies are often ſo ſwift, and conſequently the Time obſerved is ſo ſhort, that an imperceptible Error in that Time may occaſion an Error in the Velocity, thus found, of 2, 3, 4, 5, or 600 Feet in a Second. The other Method is ſo fallacious, by reaſon of the Reſiſtance of the Air, (to which Inequality the firſt is alſo liable) that the Velocities thus aſſigned may not be perhaps the tenth Part of the actual Velocities ſought.

To remedy then theſe Inconveniencies, I have invented a new Method of finding the real Velocities of Bullets of all kinds; and this to ſuch a Degree of Exactneſs, (which may be augmented too at Pleaſure) that in a Bullet moving with a Velocity of 1700 Feet in 1″, the Error in the Eſtimation of it need never amount to its five hundredth Part; and this without any extraordinary Nicety in the Conſtruction of the Machine. The Deſcription and Uſe of which is the Subject of the next Propoſition.

PROP. VIII.

To determine the Velocity, which any Ball moves with at any Diſtance from the Piece, it is diſcharged from.

THE ſimpleſt Method of effecting this is by Means of an Inſtrument like to that exhibited in the engraved Figure, where A B C D repreſents the

the Body of the Machine compoſed of the three Poles B, C, D ſpreading at Bottom, and joining together at the Top A; being the ſame with what is vulgarly uſed in the weighing and lifting of very heavy Bodies, and is called by Workmen the Triangles. On two of theſe Poles, towards their Tops, are ſcrewed on the Sockets R S; and on theſe Sockets the Pendulum E F G H I K is hung by Means of its croſs Piece E F, which becomes its Axis of Suſpenſion, and on which it muſt be made to vibrate with great Freedom. The Body of this Pendulum is made of Iron, having a broad Part at Bottom, which cannot be ſeen in this Scheme; but its entire Shape is repreſented in the annexed Figure A.

The lower Part of the Pendulum is covered with a thick Piece of Wood G K I H, which is faſten'd to the Iron by Screws. Something lower than the Bottom of the Pendulum there is a Brace O P, joining

joining the two Poles to which the Pendulum is ſuſpended; and to this Brace there is faſten'd a Contrivance M N U, made with two Edges of Steel, bearing on each other in the Line UN, ſomething in the Manner of a Drawing-pen; the Strength with which theſe Edges preſs on each other being diminiſhed or increaſed at Pleaſure, by Means of a Screw Z going through the upper Piece. There is faſten'd to the Bottom of the Pendulum a narrow Ribbon L N, which paſſes between theſe Steel Edges, and which afterwards, by Means of an Opening cut in the lower Piece of Steel, hangs looſely down, as at W.

This Inſtrument thus fitted, if the Weight of the Pendulum be known, and likewiſe the reſpective Diſtances of its Centre of Gravity, and of its Centre of Oſcillation, from its Axis of Suſpenſion, it will thence be known, what Motion will be communicated to this Pendulum by the Percuſſion of a Body of a known Weight moving with a known Degree of Celerity, and ſtriking it in a given Point; that is, if the Pendulum be ſuppoſed at Reſt before the Percuſſion, it will be known, what Vibration it ought to make in Conſequence of ſuch a determined Blow; and, on the contrary, if the Pendulum, being at Reſt, is ſtruck by a Body of a known Weight, and the Vibration, which the Pendulum makes after the Blow, is known, the Velocity of the ſtriking Body may from thence be detemined.

Hence then, if a Bullet of a known Weight ſtrikes the Pendulum, and the Vibration, which the Pendulum makes in conſequence of the Stroke, be aſcertained, the Velocity, with which the Ball moved, is thence to be known.

Now the Extent of the Vibration, made by the Pendulum after the Blow, may be meaſured to great Accuracy by the Ribbon L N; for let the Preſſure of the Edges U N on the Ribbon be ſo regulated

by

by the Screw Z, that the Motion of the Ribbon between them may be free and eaſy, tho' with ſome minute Reſiſtance; then ſettling the Pendulum at Reſt, let the Part L M between the Pendulum and the Edges be drawn ſtrait, but not ſtrained, and fix a Pin in that Part of the Ribbon, which is then contiguous to the Edges: Let now a Ball impinge on the Pendulum, then the Pendulum ſwinging back will draw out the Ribbon to the juſt Extent of its Vibration, which will conſequently be determined by the Interval on the Ribbon between the Edges U N and the Place of the Pin.

But the Computation, by which the Velocity of the Ball is determined from the Vibration of the Pendulum after the Stroke, requires a more particular Explication; and for this Purpoſe we will exhibit, as an Example, the Pendulum made uſe of by us in ſome of our Experiments.

The Weight of the whole Pendulum, Wood and all, was 56 lb. 3 oz. its Centre of Gravity was 52 Inches diſtant from its Axis of Suſpenſion, and 200 of its ſmall Swings were performed in the Time of 253 Seconds; whence its Centre of Oſcillation (determined from hence) is 62 $\frac{2}{3}$ Inches diſtant from that Axis. The Centre of the Piece of Wood G K I H is diſtant from the ſame Axis 66 Inches.

In the compound Ratio of 66 to 62 $\frac{2}{3}$, and 66 to 52, take the Quantity of Matter of the Pendulum to a 4th Quantity, which will be 42 lb. $\frac{1}{3}$ oz. Now Geometers will know, that if the Blow be ſtruck in the Centre of the Piece of Wood G K I H, the Pendulum will reſiſt to the Stroke in the ſame Manner, as if this laſt Quantity of Matter only (42 lb. $\frac{1}{3}$ oz.) was concentrated in that Point, and the reſt of the Pendulum was taken away; whence, ſuppoſing the Weight of the Bullet impinging in that Point to be the $\frac{1}{12}$ of a Pound, or the $\frac{1}{504}$ of this Quantity of Matter

Matter nearly, the Velocity of the Point of Percuſſion after the Stroke will, by the Laws obſerved in the Congreſs of ſuch Bodies as rebound not from each other, be the $\frac{1}{505}$ of the Velocity the Bullet moved with before the Stroke; whence the Velocity of this Point of Percuſſion after the Stroke being aſcertained, that multiplied by 505 will give the Velocity with which the Ball impinged.

But the Velocity of the Point of Percuſſion after the Stroke is eaſily deduced from the Chord of the Arch, through which it aſcends by the Blow; for it is a well known Propoſition, that all pendulous Bodies aſcend to the ſame Height by their vibratory Motion, as they would do, if they were projected directly upwards from their loweſt Point, with the ſame Velocity they have in that Point; wherefore, if the verſed Sine of the aſcending Arch be found, (which is eaſily determined from the Chord and Radius being given) this verſed Sine is the perpendicular Height, to which a Body projected upwards with the Velocity of the Point of Percuſſion would ariſe; and, conſequently, what that Velocity is, can be eaſily computed by the common Theory of falling Bodies.

For inſtance, the Chord of the Arch, deſcribed by the Aſcent of the Pendulum after the Stroke meaſured on the Ribbon, has been ſometimes $17\frac{1}{4}$ Inches; the Diſtance of the Ribbon from the Axis of Suſpenſion is $71\frac{1}{8}$ Inches, whence reducing $17\frac{1}{4}$ in the Ratio of $71\frac{1}{8}$ to 66, the reſulting Number, which is nearly 16 Inches, will be the Chord of the Arch, through which the Centre of the Board GKIH aſcended after the Stroke: Now the verſed Sine of an Arch, whoſe Chord is 16 Inches, and its Radius 66, is 1,93939; and the Velocity, which would carry a Body to this Height, or, which is the ſame thing, the Velocity, which a Body would acquire by deſcending

deſcending through this Space, is nearly that of $3\frac{1}{4}$ Feet in 1″.

To determine then the Velocity, with which the Bullet impinged on the Centre of the Wood, when the Chord of the Arch deſcribed by the Aſcent of the Pendulum, in conſequence of the Blow, was $17\frac{1}{4}$ Inches meaſured on the Ribbon, no more is neceſſary, than to multiply $3\frac{1}{4}$ by 505, and the reſulting Number 1641 will be the Feet which the Bullet would deſcribe in 1″, if it moved with the Velocity it had at the Moment of its Percuſſion; for the Velocity of the Point of the Pendulum, on which the Bullet ſtruck, we have juſt now determined to be that of $3\frac{1}{4}$ Feet in 1″; and we have before ſhewn, that this is the $\frac{1}{505}$ of the Velocity of the Bullet. If then a Bullet weighing $\frac{1}{12}$ of a Pound ſtrikes the Pendulum in the Centre of the Wood GKIH, and the Ribbon be drawn out $17\frac{1}{4}$ Inches by the Blow, the Velocity of the Bullet is that of 1641 Feet in 1″. And ſince the Length, the Ribbon is drawn, is always nearly the Chord of the Arch deſcribed by the Aſcent, (it being placed ſo as to differ inſenſibly from thoſe Chords which moſt frequently occur) and theſe Chords are known to be in the Proportion of the Velocities of the Pendulum acquired from the Stroke, it follows, that the Proportion between the Lengths of Ribbon drawn out at different times, will be the ſame with that of the Velocities of the impinging Bullets; and, conſequently, by the Proportion of theſe Lengths of Ribbon to $17\frac{1}{4}$, the Proportion of the Velocity, with which the Bullets impinge to the known Velocity of 1641 Feet in 1″, will be determined.

Hence then is ſhewn, in general, how the Velocities of Bullets of all kinds may be found out by Means of this Inſtrument; but that thoſe, who may be diſpoſed to try theſe Experiments, may not have unforeſeen Difficulties to ſtruggle with, I ſhall here

ſubjoin

ſubjoin a few Obſervations, which it will be neceſſary for them to attend to, both to ſecure Succeſs to their Trials, and Safety to their Perſons.

And firſt, that they may not conceive the Piece of Wood G K I H to be an unneceſſary Part of the Machine, I muſt inform them, that if a Bullet impelled by a full Charge of Powder ſhould ſtrike directly on the Iron, the Bullet would be beaten into Shivers by the Stroke, and theſe Shivers will rebound back with ſuch Violence, as to bury themſelves in any Wood they chance to light on, as I have found by hazardous Experience; and beſides the Danger, the Pendulum will not in this Inſtance aſcertain the Velocity of the Bullet, becauſe the Velocity, with which the Parts of it rebound, is unknown.

The Weight of the Pendulum, and the Thickneſs of the Wood, muſt be in ſome Meaſure proportioned to the ſize of the Bullets which are uſed. A Pendulum of the Weight here deſcribed will do very well for all Bullets under three or four Ounces, if the Thickneſs of the Board be increaſed to 7 or 8 Inches for the heavieſt Bullets; Beech is the toughest and propereſt Wood for this Purpoſe.

It is hazardous ſtanding on the ſide of the Pendulum, unleſs the Board be ſo thick, that the greateſt Part of the Bullet's Force is loſt before it comes at the Iron, for if it ſtrikes the Iron with Violence, the Shivers of Lead, which cannot return back thro' the Wood, will force themſelves out between the Wood and Iron, and will fly to a conſiderable Diſtance.

As there is no effectual way of faſtening the Wood to the Iron but by Screws, the Heads of which muſt come thro' the Board; the Bullets will ſometimes light on thoſe Screws, from whence the Shivers will diſperſe themſelves on every ſide.

When in theſe Experiments ſo ſmall a Quantity of Powder is uſed, as will not give to the Bullet a Velocity of more than 4 or 500 Feet in 1″, the Bullet will not ſtick in the Wood, but will rebound from it entire, and (if the Wood be of a very hard Texture) with a very conſiderable Velocity indeed. I have never examined any of the Bullets, which have thus rebounded, but I have found them indented by the Bodies they have ſtruck againſt in their Rebound.

To avoid then theſe Dangers, to the braving of which in Philoſophical Reſearches no Honour is annexed, it will be convenient to fix whatſoever Barrel is uſed, on a ſtrong heavy Carriage, and to fire it with a little ſlow Match. Let the Barrel too be very well fortified in all its Length; for no Barrel, (I ſpeak of Muſket Barrels) forged with the uſual Dimenſions will bear many of the Experiments recited hereafter without burſting, as I have found to my Coſt. The Barrel, I have moſt relied on, and which I procured to be made on Purpoſe, is nearly as thick at the Muzzel as at the Breech, that is, it has in each Place nearly the Diameter of its Bore in Thickneſs of Metal.

The Powder uſed in theſe Experiments ſhould be exactly weigh'd, and that no Part of it be ſcattered in the Barrel, the Piece muſt be charged with a Ladle in the ſame manner as is practiſed with Cannon, the Wad ſhould be of Tow of the ſame Weight each Time, and no more than is juſt neceſſary to confine the Powder in its proper Place, the Length of the Cavity left behind the Ball ſhould be determined each Time with Exactneſs, for the increaſing or diminiſhing that Space will vary the Velocity of the Shot, altho' the Bullet and Quantity of Powder be not changed. The Diſtance of the Mouth of the Piece from the Pendulum ought to be ſuch, that the Impulſe of the Flame may not act on the Pendulum; this

this will be prevented in a common Barrel charged with ¼ an Ounce of Powder, if it be at the Diſtance of 16 or 18 Feet; in larger Charges the Impulſe is ſenſibly farther off, I have found it extend to above 25 Feet; however, between 25 and 18 Feet is the Diſtance I have uſually choſen; other Precautions, which are neceſſary, will better find their Place in the Account of the Experiments I have made, to which I now haſten.

PROP. IX.

To compare the actual Velocities with which Bullets of different kinds are diſcharged from their reſpective Pieces, with their Velocities computed from the Theory.

HOW to determine the actual Velocities with which Bullets are diſcharged, has been amply ſhewn in the laſt Propoſition; and how to compute the Velocity with which they would be diſcharged according to our Theory, has been likewiſe fully explained in the ſixth Propoſition; we ſhall here then compare the Reſult of our Theory with Experience, and thence evince, how accurately that Theory agrees with the real Motions of Bullets, tho' founded on Principles no ways connected with theſe Experiments.

The firſt Experiments, I ſhall exhibit, were made with a Barrel of the ſame Dimenſions with the Example of the ſixth Propoſition, the Ball being ¼ of an Inch in Diameter, the Length 45 Inches, and the Cavity containing the Powder 2 ⅝ Inches, which, as the Barrel exceeded the Bullet in Diameter by about the $\frac{1}{40}$th of an Inch, juſt contained 12 dw. of Powder.

The Bullet thus made uſe of was $\frac{1}{12}$ of a Pound, Avoirdupoiſe, in Weight, and conſequently the ſame

with the Example of the ſeventh Propoſition; but the Board on the Pendulum uſed here was 4 lb. lighter than what is aſſigned in that Example; from theſe Circumſtances, and the Velocity which by the Theory the Bullet ought to be diſcharged with, there is known the Chord of the Arch meaſured on the Ribbon, thro' which the Pendulum ſhould aſcend after the Stroke, if the Theory be true: How near this agrees with our Experiments, will appear by the following Table:

No.	Quantity of Powder.	Chord of aſcending Arch meaſured on the Ribbon.	The ſame by the Theory.	Error of Theory.
	Dw.			
1	12	18,7	19,0	+,3
2	12	19,6	19,0	—,6
3	6	13,6	13,4	—,2

The next Experiments were made with the ſame Barrel, but the Board on the Pendulum was now of little more Weight than that in the Example of the ſeventh Propoſition.

No.	Length of the Cavity containing the Powder or Line AF, in Fig. 1.	Quantity of Powder.	Chord of aſcending Arch meaſured on the Ribbon.	The ſame by Theory.	Error of Theory.
	Inches.	Dw.	Inch.	Inch.	Inch
4	2 $\frac{5}{8}$	6	11,9	12,1	+,2
5	2 $\frac{5}{8}$	6	12,2	12,1	—,1
6	1 $\frac{1}{4}$	6	13,2	13,6	+,4
7	1 $\frac{1}{4}$	6	13,9	13,6	—,3
8	2 $\frac{5}{8}$	12	16,7	17,2	+,5
9	2 $\frac{5}{8}$	12	17,5	17,2	—,3
10	2 $\frac{5}{8}$	12	16,9	16,8	—,1
11	2 $\frac{5}{8}$	12	17,0	16,8	—,2
12	2 $\frac{5}{8}$	6	11,7	11,5	—,2
13	2 $\frac{5}{8}$	6	11,1	11,5	+,4
14	2 $\frac{5}{8}$	12	16,7	16,3	—,4

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The laſt five Numbers reſulting from the Theory are corrected from the Quantity of Bullets lodged in the Board, which, as many other Experiments of a different kind were tried in the Interval, amounted at laſt to above two Pounds; whence the Weight of the Pendulum being increaſed, its Vibration with the ſame Blow muſt be proportionably diminiſhed.

The next Experiments were made with a Barrel of the ſame Bore with the laſt, but only 12, 375 Inches in Length: To diſtinguiſh them, we ſhall for the future denominate the firſt Barrel by the Letter A, and this ſhort one by C. The Board on the Pendulum was at firſt rather lighter than in the Example of the ſeventh Propoſition.

No.	Barrel	Extent of the Cavity containing the Powder.	Quantity of Powder.	Chord of aſcending Arch meaſured on the Ribbon.	The ſame by Theory.	Error of Theory.
		Inch.	Dw.	Inch.	Inch.	
15	C	2 $\frac{5}{8}$	12	12,7	12,8	+, 1
16	C	2 $\frac{5}{8}$	12	12,6	12,8	+, 2
17	C	2 $\frac{5}{8}$	12	12,4	12,8	+, 4
18	A	2 $\frac{5}{8}$	12	17,0	17,3	+, 3
19	A	2 $\frac{5}{8}$	12	17,2	17,2	,0
20	A	2 $\frac{5}{8}$	12	17,1	17,2	+, 1
21	A	2 $\frac{5}{8}$	12	17,2	17,2	,0
22	A	2 $\frac{5}{8}$	6	12,4	12,2	—, 2

In ſome of the following Experiments a third Barrel was uſed of the ſame Bore with the other two, but 24.312 Inches in Length: This Barrel I denominate B; the Board fixed on the Pendulum was at firſt but little heavier than that in the ſeventh Propoſition; and when in the Courſe of the Experiments it is ſenſibly increaſed in the Weight, I diminiſh the Numbers ariſing from the Theory by a correſponding Part.

No.	Barrel.	Extent of the Cavity containing the Powder.	Quantity of Powder.	Chord of aſcending Arch meaſured on the Ribbon.	The ſame by Theory.	Error of Theory.
		Inch.	dw.	Inch.	Inch.	
23	A	2 5/8	12	17, 1	17, 2	+, 1
24	A	2 5/8	9	15, 2	15, 0	—, 2
25	A	2 5/8	9	15, 4	15, 0	—, 4
26	C	2 5/8	12	11, 5	12, 8	+1,3
27	C	2 5/8	12	11, 5	12, 8	+1,3
28	C	2 5/8	6	8, 7	9,	+, 3
29	C	2 5/8	12	12, 3	12, 5	+, 2
30	B	2 5/8	12	14, 4	14, 4	0,0
31	B	2 5/8	12	14, 4	14, 4	0,0
32	B	2 5/8	6	10, 3	10, 5	+, 2
33	A	1 1/4	8	14, 7	14, 5	—, 2
34	A	4	12	15, 7	15, 3	—, 4

The Error in the 26th and 27th Experiments being much greater than what has occurred to me in any other Trials, I ſuſpect, that ſome Miſtake was made in the Weight of the Powder, or that the Barrel (which had indeed lain by in a moiſt Place) was very damp; which Circumſtance, I know by Experience, will conſiderably diminiſh the Action of the Powder.

The following Experiments were made with a Pendulum much heavier, it weighing in the whole 97 lb. its Center of Gravity was 55,625 Inches diſtant from its Axis of Suſpenſion, and 200 of its ſmall Swings were performed in the Space of 255″ 1/4, whence its Center of Oſcillation is 63,9 Inches diſtant from the Axis of Suſpenſion. Alſo ſometimes another Barrel was uſed 7,06 Inches in Length, and ,83 in Diameter, its Ball was exactly fitted to the Bore without any Windage, ſo that it went in with Difficulty, the Weight of this Ball was 33 1/2 dw. This Barrel we ſhall denominate D 2

No	Barrel	Length of the Cavity containing the Powder.	Quantity of Powder.	Chord of the afcending Arch meafured on the Ribbon.	The fame by the Theory.	Error of Theory.
		Inch.	dw.	Inch.	Inch.	Inch.
35	A	2 5/8	12	9, 2	9, 2	,0
36	A	2 5/8	12	9, 5	9, 2	—,3
37	A	5 1/4	24	11, 7	11,3	—,4
38	A	7 7/8	36	13, 2	12,6	—,6
39	A	2 5/8	12	9, 3	9, 1	—,2
40	A	1 3/4	8	7, 6	8, 1	+,5
41	C	2 5/8	12	6, 1	6, 6	+,5
42	C	2 5/8	12	6, 5	6, 6	+,1
43	B	2 5/8	12	8, 0	8, 2	+,2
44	B	2 5/8	12	8, 3	8, 2	—,1
45	A	2 5/8	12	9, 5	9, 1	—,4
46	A	2 5/8	12	9, 1	9, 1	,0
47	A	2 5/8	6	7, 2	6, 5	—,7
48	A	2 5/8	6	6, 7	6, 5	—,2
49	C	2 5/8	12	6, 8	6, 7	—,1
50	C	2 5/8	12	7, 5	6, 7	—,8
51	C	2 5/8	6	4, 7	4, 8	+,1
52	C	2 5/8	6	5, 0	4, 8	—,2
53	D	2 1/4	12	7, 0	7, 2	+,2
54	D	2 5/8	12	7, 1	6, 8	—,3
55	D	2 5/8	6	4, 7	4, 8	+,1
56	D	2 5/8	6	4, 8	4, 8	,0
57	A	2 1/16	6	6, 4	6, 5	+,1
58	A	2 1/16	6	6, 4	6, 5	+,1
59	A	2 1/16	6	6, 6	6, 5	—,1
60	A	2 1/16	6	6, 7	6, 5	—,2
61	A	2 1/16	12	9, 0	9, 1	+,1

The Error in the 50th Experiment, the greateſt in this Set, was doubtleſs owing to the Wind; for the 49, which was made immediately before it in the ſame Manner, and with the ſame Quantity of Powder, differs but little from the Theory. The Exceſs of the 38th Experiment above the Theory was in Part occaſioned by the Impulſe of the Flame on the Pendulum, which in this large Quantity of Powder was plainly to be diſcerned.

This Theory is farther confirmed too, by Experiments made on the Action of very ſmall Quantities of Powder. We have hitherto ſuppoſed the Powder when fired to be equally hot with Iron at the begining of its white Heat, but we have obſerved, that in very ſmall Quantities of Powder the Heat is probably leſs than this, and conſequently the Elaſticity in thoſe Caſes leſs than what ariſes from this Suppoſition. Now this Decreaſe of Elaſticity in ſmall Quantities of Powder we have found by many Trials actually to take place; for Inſtance, in the Example of the 7th Propoſition, the Velocity, which ſhould be given to the Ball by the Action of the Powder according to the Theory, is in round Numbers that of 1670 Feet in 1″, and this Velocity we have found in the preceeding Experiments to be the medium Velocity, which the Ball really receives in thoſe Circumſtances. If now, the Barrel and Poſition of the Ball remaining the ſame, there be placed in the ſame Space D E G C continuing likewiſe the ſame 1dw. of Powder inſtead of 12, which is the Quantity ſuppoſed in that Example, it follows from the Principles there laid down, that if the Elaſticity of the ſmaller Charge be the ſame in Proportion to its Quantity with that of the larger, then the Velocity of the Bullet when impelled by the Exploſion of the ſmaller Charge, will be to the Velocity of a Bullet impelled by the greater Charge in the ſubduplicate Ratio of the Quantities of the reſpective Charges,

Charges, that is, in the ſubduplicate Ratio of 1 to 12. Conſequently the Velocity communicated by 12 dw. being known to be that of 1670 Feet in 1″, the Velocity communicated by 1 dw. would be that of 482 Feet in 1″ nearly; but I found by repeated Trials, which differed very little from each other, that the real Velocity acquired by the Ball in this Caſe, from the Exploſion of 1 dw. was rather leſs than that of 400 Feet in 1″; whence it is evident, that the Elaſticity of 1 dw. of Powder when fired is leſs in Proportion to its Quantity than that of 12 dw. as it ought to be by our Theory.

Likewiſe if 3 dw. of Powder be placed in the ſame Manner with the 1 dw. in the laſt Example, the real Velocity, the Ball will acquire from the Exploſion, will be from 740 to 720 Feet in 1″, as I have found by many Experiments; whereas ſuppoſing the Elaſticity of 3 dw. when fired to be in ſimilar Circumſtances the ſame with that of 12 dw. the Velocity acquired by the Ball ſhould be 835 Feet in 1″; whence, even in 3 dw. the Elaſticity and conſequently the Heat is leſs than in 12 dw. as it ought to be by the Theory.

The Ratios of theſe diminiſhed Elaſticities to the Elaſticity eſtabliſhed by us for larger Quantities, are the Duplicate of the Ratios of the reſpective Velocities communicated by them in acting thro' the ſame Space, whence the Elaſticity of 1 dw. is to the Elaſticity of 12 dw. in ſimilar Circumſtances as 2 to 3. and the Elaſticity of 3 dw. to that of 12 dw. is as 3 to 4 nearly, ſuppoſing the ſmaller Elaſticities to be uniformly diminiſhed in each Part of their Expanſion; but in all Probability this is not the Caſe, for as the Decreaſe of Elaſticity is owing to a Deficiency in the Heat of the Fluid, it is moſt reaſonable to conceive, that in theſe ſmall Quantities the Heat is not only leſs at firſt, but is likewiſe perpetually decreaſing, as the Bullet is impelled thro'

the Barrel, and conſequently that the comparative Elaſticity is more abated, the longer its Action continues; a Circumſtance that does not take place, when the Quantity of Powder is well proportioned to the Barrel, thro' which it is to act.

SCHOLIUM.

Thus then have we confirmed our Theory by the moſt indiſputable Evidence, which is its Coincidence with a numerous Series of Experiments made in all the various Circumſtances, that the Nature of the Subject could ſuggeſt; and I muſt add, that theſe Experiments or the greateſt Part of them were made and regiſter'd before any of the Computations, by which they are compared with the Theory, were formed; altho' I had aſcertained that Theory, ſuch as it is here delivered, ſome time before I enter'd on theſe Experiments.

The Variety of theſe Experiments, and the Accuracy with which they correſpond to the Theory, leave us no room to doubt of the Certainty of the Theory; for we have examined the Action of fired Powder on Bullets of different Weights and in Barrels of different Magnitudes from 7 Inches to 45 Inches in Length; we have varied too the Quantity of Powder from 6 dw. to 36 dw. and have incloſed theſe Quantities differently, ſometimes in ſuch Cavities as they exactly filled, at other times in ſuch as were ſufficient to have held much greater Quantities; and we have found, that in all theſe different Circumſtances, our Theory has truly exhibited the real Velocities with which the Bullets were impelled by the Exploſion, or if (as in very ſmall Quantities of Powder) there were any Exceptions from the general Rule we had eſtabliſhed, it was, what the Theory did plainly demand; and ſurely that Theory, which ſo accurately agrees with Experiment in theſe diverſified Trials, cannot but contain

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the true and genuine Determination of the Force and Manner of acting of fired Gunpowder.

This Theory as here eſtabliſhed ſuppoſes, that in the firing of Gunpowder about $\frac{1}{10}$ of its Subſtance is converted by the ſudden Inflammation into a permanent elaſtic Fluid, whoſe Elaſticity in Proportion to its Heat and Denſity is the ſame with that of common Air in the like Circumſtances; it farther ſuppoſes, that all the Force exerted by Gunpowder in its moſt violent Operations is no more than the Action of the Elaſticity of the Fluid thus generated; and theſe Principles enable us, as we have ſeen, to determine the Velocities of Bullets impelled from Fire-Arms of all kinds, and are fully ſufficient for all Purpoſes, where the Force of Gunpowder is to be eſtimated. Whether this Fluid be true and genuine Air, or another Subſtance, we ſhall not diſcuſs in this Place, as it is an Inquiry no Ways connected with the Deſign of this Treatiſe.

From this Theory many Deductions may be made of the greateſt Conſequence to the Practical Part of Gunnery. From hence the Thickneſs of a Piece, which will enable it to confine without Burſting any given Charge of Powder, is eaſily determined, ſince the Effort of the Powder is known. From hence appears the Inconcluſiveneſs of what ſome modern Authors have advanced relating to the Advantages of particular Forms for the Chambers of Mortars and Cannon; for all their labour'd Speculations on this Head are evidently founded on very erroneous Opinions about the Action of fired Powder. From this Theory too we are taught the Neceſſity of leaving the ſame Space behind the Bullet, when we would by the ſame Quantity of Powder communicate the ſame Velocity to the Bullet; ſince on our Principles it follows, that the ſame Powder has a greater or leſs Degree of Elaſticity, according to the different Spaces it occupies. The Method,

which

which I have always practiſed for this Purpoſe, has been by marking the Rammer, and this is a Maxim, which ought not to be diſpenſed with, when Cannon are fired at an Elevation, particularly in thoſe called by the French *Batteries à Ricochet*.

From the continued Action of the Powder, and its Manner of expanding deſcribed in this Theory, and the Length of the Piece, one of the moſt eſſential Circumſtances in the well directing of Artillery may be eaſily aſcertained. All Practitioners are agreed, that no Shot can be depended on, unleſs the Piece be placed on a ſolid Platform; for if the Platform ſhakes with the firſt Impulſe of the Powder, it is impoſſible, but the Piece muſt likewiſe ſhake, which will alter its Direction and render its Shot uncertain. To prevent this Accident, the Platform is uſually made extremely firm to a conſiderable Depth backwards, ſo that the Piece is not only well ſupported in the Beginning of its Motion, but likewiſe thro' a great Part of its Recoil. However, it is ſufficiently obvious, that when the Bullet is ſeparated from the Piece, it can be no longer affected by the trembling of the Piece or Platform; and by a very eaſy Computation it will be found, that in a Piece 10 Feet in length, carrying a Bullet of 24 lb. and charged with 16 lb. of Powder, the Bullet will be out of the Piece before the Piece has recoil'd $\frac{1}{4}$ an Inch; whence, if the Platform be ſufficiently ſolid at the Beginning of the Recoil, the remaining Part of it may be much ſlighter, ſince its Unſteadineſs beyond the firſt $\frac{1}{4}$ Inch will have no Influence on the Direction of the Shot. And hence a more compendious Method of conſtructing Platforms may be found out.

From this Theory it alſo appears, how greatly thoſe Authors have been miſtaken, who have attributed the Force of Gunpowder, or at leaſt a conſiderable Part of it, to the Action of the Air, contained either in the Powder or between the Intervals of the Grains:

Grains. For they have ſuppoſed (tho' indiſtinctly enough) that Air to exiſt in its natural elaſtic State, and to receive all its Addition of Force from the Heat of the Exploſion. But, from what we have experimented in the fifth Propoſition, relating to the Increaſe of the Elaſticity of the Air by Heat, we may conclude, that the Heat of the Exploſion cannot augment the Elaſticity of the Air to five times its common Quantity; conſequently, the Force ariſing from this Cauſe only cannot amount to more than the 200th Part of the real Force exerted on this Occaſion.

Having thus diſpatched the general Confirmation of our Theory, we ſhall proceed to the Examination of ſome other Particulars relating to this Subject, which tho' eaſily enough flowing from the Principles already laid down, do yet from the Novelty and Singularity of the Matter merit a circumſtantial Diſcuſſion.

PROP. X.

To aſſign the Changes in the Force of Powder, which ariſe from the different State of the Atmoſphere.

IN all the Experiments I have hitherto examined, I have never been able to diſcover, that the Variation of the Denſity of the Atmoſphere did any way alter the Action of Powder, altho' I have made ſeveral hundred Shot in very different Seaſons: In particular, I have ſometimes compared the Trials made at Noon in the hotteſt Summer Sun with thoſe made in the Freſhneſs of the Morning and Evening, and I could not perceive any certain Difference between them, and it was the ſame with the Trials made in the Night and in Winter; altho' in this variety of Seaſons, the Denſity of the Atmoſphere muſt

muſt have been very different: Indeed, as we have ſeen, that the ſame Quantity of that Fluid, in which the Force of Powder conſiſts, is generated in a *vacuum*, and in common Air, it is difficult to conceive, how this Force can be affected by the greater or leſſer Denſity of the Atmoſphere.

But tho' the Denſity of the Atmoſphere has no Influence on the Force of Powder, yet its Moiſture has a great one; for the ſame Quantity of Powder, which in a dry ſeaſon would communicate to a Bullet a Velocity of 1700 Feet in 1″, will not in damp Weather communicate to the ſame Bullet, placed in the ſame manner, a Velocity of more perhaps than 12 or 1300 Feet in 1″, or ſtill leſs, if the Powder be of a bad Sort, and has been negligently kept. And this Decreaſe of the Force in damp Powder appears by my Experiments to be very unſteady and variable, ſo that 2 Shot made with equal Quantities of ſuch Powder, taken out of the ſame Parcel, will differ conſiderably from each other, perhaps ten times more than if the Powder was in good Order; and as far as this Uncertainty in its Effects will permit me, I ſeem to collect, that a ſmall Charge loſes a greater Part of its Force than a larger, provided each are equally damp. Another Circumſtance attending damp Powder is a remarkable Foulneſs in the Piece after firing, much beyond what ariſes from an equal Quantity of dry Powder.

Now all theſe Effects are eaſily to be accounted for, when it is known, that Powder will imbibe Moiſture from the Air; for as a certain Quantity of Water mixed with Powder will prevent its firing at all, it follows, that every Degree of Moiſture in Powder, tho' inſufficient to produce this Effect, will yet abate the Violence of its Exploſion, and will render the Fire thereby produced leſs vehement than it would otherwiſe be; whence a leſs Quantity

of

of Fluid will be in this Caſe generated, and the Heat of that Fluid and its Elaſticity is likewiſe leſs; conſequently, the Action of damp Powder muſt on this two-fold Account be diminiſhed according to the Degree of Moiſture, with which it is impregnated.

And as bad Powder uſually contains ſome common Salt in it, by reaſon of the little Care taken in the refining of the Nitre, and as common Salt imbibes Moiſture in a ſtronger Degree than Nitre, it is not difficult to conceive, how bad Powder ſhould in a moiſt Seaſon be more impregnated with Moiſture than good, and ſhould conſequently loſe more of its Force.

The Uncertainty in the Effects of damp Powder ariſes, I preſume, from the different Degrees of Dryneſs it acquires in the Piece; for as after the firſt or ſecond firing the Barrel grows warm, if the Powder is contained any Time in it, ſome Part of its Moiſture will be thereby evaporated, and as the Heat of the Barrel, and the Time of the Charge continuing in it, are Circumſtances, which in their Nature are very uncertain, it is not to be wonder'd at, that the Evaporation, and conſequently the Action of the Powder, is likewiſe uncertain. I muſt remark on this Head, that, in the drieſt Seaſons, I have found the Coldneſs of the Barrel, and perhaps ſome little Moiſture condenſed in its Cavity, to have ſenſibly diminiſhed the Force of the Powder in the firſt Shot.

That ſmall Quantities of Powder ſhould have their Action more diminiſhed than larger Quantities with the ſame Degree of Moiſture, naturally follows from the ſmaller Degree of Heat, with which (as we have obſerved above) the Exploſion of ſmall Charges is attended; ſince the ſame Proportion of Moiſture muſt of neceſſity clog a weak Fire more effectually, than it can do one which is more violent.

The remarkable Foulneſs of the Piece from the firing of damp Powder, which we have mentioned above, muſt likewiſe ariſe from the Diminution of the

the Activity of the Fire in the Exploſion. For, when Powder is of a proper Temperature to fire readily and violently, the greateſt Part of its Subſtance ought to be conſumed to Aſhes, which will then be diſcernable in the Form of a greyiſh Subſtance on all Bodies placed near the Mouth of the Piece; and the Foulneſs of the Piece is owing to thoſe Parts of the Powder, which either by their Contiguity to the cold Barrel, or their leſs inflammable Compoſition, are but imperfectly burnt; now ſince moiſt Powder produces a leſs violent Flame in Proportion to the Moiſture it imbibed, it muſt follow, that a ſmaller Part of the Powder will in this Caſe be perfectly conſumed, and conſequently a greater Part will remain to contribute to the Foulneſs of the Barrel.

SCHOLIUM.

We have aſſerted as the Baſis of our Reaſoning in this Propoſition, that Powder will imbibe Moiſture from the Air in a humid State of the Atmoſphere; but it remains to aſſign the Quantity it can thus imbibe, which we ſhall here endeavour to do from our own Experiments.

A Parcel of very good Powder being placed on a white Paper, which was pierced with a great Number of fine Holes, and the Paper being held over the Steam of hot Water, I found, that in half a Minute the Powder was increaſed in Weight by about $\frac{1}{50}$ Part.

Trying another Parcel in the ſame Manner, but continuing it longer in the Steam, I found, that the Powder increaſed its Weight by $\frac{1}{24}$ Part; but in this Caſe ſome of the Grains adhered together in ſmall Lumps, altho' the Figure of the Grains themſelves was no ways changed.

To convince myſelf that the Moiſture of the Atmoſphere would likewiſe increaſe the Weight of Powder, I took about an Ounce of Powder, which had

had for ſome time been kept in a Room which had a Fire in it every Day, and I found by drying it before the Fire, that it loſt above $\frac{1}{100}$ of its Weight; one third of which Decreaſe in Weight it had again acquired in leſs than two Hours, by being removed to a different Part of the Room, at a Diſtance from the Fire.

Now as the Weather is often much moiſter, than when I tried this Experiment, and as in open Air this Moiſture abounds much more than in a Room, where there is a Fire; it cannot be doubted, but that ſometimes the twentieth or thirtieth Part of the Subſtance of the beſt Powder is Water, which may be eaſily ſuppoſed to produce all the Effects, we have obſerved and deſcribed in this Propoſition.

But however, the Moiſture thus imbibed by Powder from the Air does not, as I have yet obſerved, render it leſs active, when it is dried again. The Reader muſt have obſerved in the Experiments of the laſt Propoſition, how nearly thoſe made with the ſame Quantities of Powder and in the ſame Circumſtances agree with each other. In theſe Experiments, tho' made at different Times in the Courſe of three Summer Months, the Dryneſs of the Seaſon prevented all the Inequalities of this Propoſition. But trying the ſame Powder in the Winter, in a very damp Seaſon, I found, that tho' if it was uſed as in the Summer, in its natural State, without any drying, its Effects were very irregular, and much ſhort of thoſe Experiments, yet if each Charge was well dried, juſt before it was uſed, no Diminution of its Force could then be perceived, nor did it appear to act in any manner different from what it had done in the preceeding Summer. Indeed if the Powder be expoſed to the greateſt Damps without any Caution, or if common Salt abounds in it, the Moiſture, it imbibes, may perhaps be ſufficient to diſſolve ſome Part of the

the Nitre, which is a laſting Damage, that no Drying can retrieve. But when tolerable Care is taken in preſerving Powder, and the Nitre, it is compoſed of, has been well purged from common Salt, it will retain its Force much longer than is uſually ſuppoſed. I have heard, that Powder, which had been well kept, did not at the End of fifty Years appear to be any ways injured by its Age.

Some Care is neceſſary in the drying of damp Powder; for there is a Degree of Heat, which tho' not ſufficient to fire the Powder, will yet melt the Brimſtone, and deſtroy the Texture of the Grains. Nay more, there is a Heat, with which the Brimſtone will flame and burn away gradually, and yet the Powder will not explode; of this any one may ſatisfy himſelf by heating a Piece of Iron red-hot, and then throwing a few Grains of Powder on it at different Intervals, during the Time of its cooling, for by this Means he will find, that at a certain Time the ſeparate Grains, that fall on the Iron, will not explode, but will burn with a ſmall blue Flame for ſome Space of Time, the Grain ſtill remaining unconſumed. Indeed, when it has begun to burn in this Manner, it ſometimes ends with exploding, but this more commonly happens, when a Number of Grains lie near together; for then tho' each ſeparate Flame is not ſufficient to explode its reſpective Grain, yet the whole Fire made by them all together grows ſtrong enough at laſt to end in a general Exploſion; however, by attending to the proper Temperature of the Iron, and ſpreading the Grains, I have often covered two or three ſquare Inches with a blue lambent Flame, which has laſted a conſiderable Time without any Exploſion, and examining the Grains afterwards, I could not perceive, that they had loſt either their Colour or their Shape. Now ſince theſe Grains, when the Brimſtone is thus burnt or even melted out of them, will no longer act as Powder,

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it is evident, that Powder may be ſpoiled by being dried with too violent a Heat.

From the great Difference in the Effects of moiſt and dry Powder eſtabliſhed in this Propoſition, it appears, how very uncertain and irregular all thoſe practical Operations of Gunnery may prove, where this Circumſtance is not attended to; and how little Confidence can be placed in any Experiments, where this Cauſe of Inequality could interfere.

Before I leave this Article, I muſt mention a Suſpicion, I once entertained about this Matter. As Water, when rarified into Vapour, is generally ſuppoſed to be near ten times more elaſtic than Air equally heated, I imagined, that poſſibly the Moiſture imbibed by Powder might, in certain Caſes, be ſo proportioned to the Quantity of Powder, that it might be converted into Vapour by the Exploſion; and that thereby the Force of the Powder might be more increaſed by the Addition of this very elaſtic Vapour, than it was diminiſhed by the damping of its Flame. And I was the more induced to believe, that this did ſometimes happen, from the Experiments of a late Author, who tells us, that the Ranges of the ſame Shot, fired from the ſame Mortar, with equal Charges of Powder, were much greater in the Freſhneſs of the Morning, than in the Heat of the Day. For I was well ſatisfied, that the meer Denſity of the Air (to which he ſeems to impute this Variety) could not produce ſuch different Effects. However, upon a more accurate Examination, I cannot find, that any Degree of Moiſture does at any time augment the Force of Powder; for, in all the numerous Trials I have made, I never obſerved that Force ſenſibly to exceed its mean Quantity, except in two Experiments; and even thoſe Exceſſes, I had good Reaſon to believe, were occaſioned by ſome Diſorder in the Machine.

However, if the Elaſticity of watry Vapour be as great as it is uſually eſteemed, (a Point far from being aſcertained at preſent) it is not impoſſible, but ſomething of this kind may take Place in the firing of large Quantities of Powder.

PROP. XI.

To inveſtigate the Velocity which the Flame of Gunpowder acquires, by expanding itſelf, ſuppoſing it be fired in a given Piece of Artillery, without either a Bullet or any other Body before it.

IF the whole Subſtance of the Powder was converted into an elaſtic Fluid at the Inſtant of the Exploſion, then from the known Elaſticity of this Fluid aſſigned by our Theory, and its known Denſity, we could eaſily determine the Velocity, with which it would begin to expand, and could thence trace out its future Augmentations in its Progreſs through the Barrel; but as we have ſhewn, that the elaſtic Fluid, in which the Activity of the Gunpowder conſiſts, is only $\frac{3}{10}$ of the Subſtance of the Powder, the remaining $\frac{7}{10}$ will in the Exploſion be mixed with the elaſtic Part, and will by its Weight retard the Activity of the Exploſion; and yet they will not be ſo compleatly united as to move with one common Motion, but the unelaſtic Part will be leſs accelerated than the reſt, and ſome of it will not even be carried out of the Barrel, as appears by the conſiderable Quantity of unctuous Matter, which adheres to the Inſide of all Fire-Arms, after they have been uſed.

Theſe Inequalities in the expanſive Motion of the Flame oblige us to recur to Experiments for its accurate Determination.

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The Experiments made uſe of for this Purpoſe were of two kinds: The firſt was made by chargeing the Barrel A with 12 dw. of Powder, and a ſmall Wad of Tow only; and then placing its Mouth 19 Inches from the Centre of the Pendulum, mentioned in the ſeventh Propoſition; on firing it in this Situation, the Impulſe of the Flame on the Pendulum made it aſcend through an Arch, whoſe Chord was 13, 7 Inches; whence, if the whole Subſtance of the Powder was ſuppoſed to ſtrike againſt the Pendulum, and each Part to ſtrike with the ſame Velocity, that common Velocity muſt have been at the Rate of about 2650 Feet in 1″. This then is the leaſt Velocity, which the Powder could be ſuppoſed to acquire in its Expanſion; for if we ſuppoſe the elaſtic Part to acquire a greater Velocity in expanding, than the other groſs Vapour, (which it undoubtedly does) this common Velocity here aſſigned muſt be augmented for the elaſtic Fluid, and diminiſhed for the groſſer Subſtance of the Powder. As ſome Part of the Velocity of the Flame was loſt in paſſing through 19 Inches of Air, I made the remaining Experiments on this Subject in a manner not liable to that Inconvenience.

I fixed the Barrel A on the Pendulum, ſo that its Axis might be both horizontal and alſo perpendicular to the Plane HK; or, which is the ſame thing, that it might be in the Plane of the Pendulum's Vibration; the Height of the Axis of the Piece above the Centre of the Pendulum was ſix Inches, and the Weight of the Piece and of the Iron that faſten'd it, *&c.* was 11 lb. ¼. The Barrel in this Situation being charged with 12 dw. of Powder, without either Ball or Wad, the Powder only put together with the Rammer, on the Diſcharge the Pendulum aſcended through an Arch, whoſe Chord was 10 Inches, or reduced to an equivalent Blow in the Centre of the Pendulum, ſup-

poſing the Barrel away, it would be 14, 4 Inches nearly.

The ſame Experiment repeated again, the Chord of the aſcending Arch was 10, 1 Inches, which, reduced to the Centre, is 14, 6 Inches.

To determine what Difference of Velocity there was in the different Parts of the Vapour, I loaded the Piece again with 12 dw. of Powder, and rammed it down with a Wad of Tow weighing 1 dw. Now I conceived, that this Wad being very light would preſently acquire that Velocity, with which the elaſtic Part of the Fluid would expand itſelf when uncompreſſed; and I accordingly found, that the Chord of the aſcending Arch was by this Means augmented to 12 Inches, or at the Centre to 17,3: Whence, as the Medium of the other two Experiments is 14,5, the Pendulum aſcended thro' an Arch 2, 8 Inches longer, by the additional Motion of 1 dw. of Matter moving with the Velocity of the ſwifteſt Part of the Vapour; and conſequently, the Velocity, with which this 1 dw. of Matter moved, was that of about 7000 Feet in 1″.

It will perhaps be objected to this Determination, that the Augmentation of the Arch, through which the Pendulum vibrated in this Caſe, was not all of it owing to the Quantity of Motion given to the Wad, but Part of it was produced by the Confinement of the Powder, and the greater Quantity thereby fired. But if it were true, that a Part only of the Powder fired, when there was no Wad, it would not happen, that in firing different Quantities of Powder without a Wad, the Chord of the aſcending Arch would increaſe and decreaſe nearly in the Ratio of thoſe Quantities, which yet I have found it to do; for with 9 dw. that Chord was 7, 3 Inches, which with 12 dw. we have ſeen was but 10, and 10, 1; and even with 3 dw. the Chord was 2 Inches;

deficient

deficient from this Proportion by ,5 only; for which Defect two other valid Reaſons are to be aſſigned.

And there is ſtill a more convincing Proof, that all the Powder is fired, altho' no Wad be placed before the Charge; which is, that the Part of the Recoil ariſing from the Expanſion of the Powder alone is found to be no greater, when it impells a leaden Bullet before it, than when the ſame Quantity is fired without any Wad to confine it. We have ſeen, that the Chord of the Arch, thro' which the Pendulum roſe from the expanſive Force of the Powder alone, is 10, or 10, 1; and the Chord of that Arch, when the Piece was charged in the cuſtomary Manner with a Bullet and Wad, I found to be the firſt time 22 $\frac{1}{4}$, and the ſecond time 22 $\frac{7}{8}$; or at a Medium 22, 56. Now the Impulſe of the Ball and Wad, if they were ſuppoſed to ſtrike the Pendulum in the ſame Place, in which the Barrel was ſuſpended, with the Velocity they had acquired at the Mouth of the Piece, would drive it through an Arch, whoſe Chord would be about 12, 3, as is known from the Weight of the Pendulum, the Weight and Poſition of the Barrel, and the Velocity of the Bullet, determined by our former Experiments; whence, ſubtracting this Number 12, 3 from 22, 56, the Remainder 10, 26 is nearly the Chord of the Arch, which the Pendulum would have aſcended through, from the Expanſion of the Powder alone, when fired with a Bullet before it; and this Number 10, 26 differs very little from 10, 1, which we have above found to be the Chord of the aſcending Arch, when the ſame Quantity of Powder expanded itſelf freely, without either Bullet or Wad before it.

Again, that this Velocity of 7000 Feet in 1″ is not much beyond what the moſt active Part of the Flame acquires in expanding, is evinced from hence, that we have above, in the 38th Experiment, an

Inſtance of a Ball actually diſcharged with a Velocity of 2400 in 1″, and yet it appeared not, that the Action of the Powder on this Bullet was at all diminiſhed on account of this immenſe Celerity; conſequently, the Degree of Swiftneſs, with which in this Inſtance the Powder followed the Ball, without loſing any Part of its Preſſure, muſt have been much ſhort of what the Powder alone would have expanded with, had not the Ball been there.

And it is this prodigious Celerity of Expanſion of the Flame of fired Powder, which is its peculiar Excellence; and the Circumſtance, in which it ſo eminently ſurpaſſes all other Inventions, either ancient or modern, for the Purpoſe of military Projections: For as to the Quantity of Motion of theſe Projectiles only, many of the warlike Machines of the Ancients produced this in a Degree far ſurpaſſing that of our heavieſt Cannon-ſhot or Shells; but the great Celerity given to theſe Bodies cannot be in the leaſt approached by any other Means than by the Flame of Powder. The Reaſon of this Difference is, that the Ancients could by Weights, or the Elaſticity of Springs and ſtretched Cords, augment their Powers to any Degree deſired; but then each Addition of Power brought with it a proportional Addition of Matter to be moved: So that as the Power increaſed, thoſe Parts of the Machine, which were to communicate Motion to the Projectile, and were conſequently to move with it, were likewiſe increaſed; and thence it neceſſarily happened, that the Action of the Power was not ſolely employed in giving Motion to the impelled Body, but much the greateſt Part of it was ſpent in accelerating thoſe Parts of the Machine, in which the Power reſided, to enable them to purſue the Body to be projected with a perpetual Impulſe, during its whole Paſſage thro' the Extent of their Activity. Hence then it came to paſs, that, tho' theſe ancient

Machines

Machines could throw enormous Weights, they could project them but with ſmall Degrees of Celerity, compared with what we can communicate to our Cannon and Muſket-ſhot: Whence, in all Operations, where theſe great Velocities are uſeful, our Machines are infinitely ſuperior to thoſe of Antiquity; altho', in more confined and ſhorter Projections, theſe laſt have ſome Advantages, which may yet render them worthy of the Attention of thoſe military Genius's, who have Capacity enough to conſider each Part of their Profeſſion according to its true and genuine Value, independent of the partial Eſtimation of the Times they live in.

From the Determinations contained in this Propoſition, the Force of Petards may be deduced, ſince their Action ſolely depends on the Impulſe of the Flame; and it appears, that a Quantity of Powder, properly diſpoſed in ſuch a Machine, may produce as violent an Effort as a Bullet of twice its Weight, moving with a Velocity of 14 or 1500 Feet in 1″.

PROP. XII.

To aſcertain the Manner in which the Flame of Powder impells a Ball, which is laid at a conſiderable Diſtance from the Charge.

WE have, in many of the Experiments recited by us above, laid the Ball not immediately contiguous to the Powder, but a ſmall Diſtance from it; the greateſt Interval, however, has not amounted to more than about 1½ Inch, from the hinder Part of the Bullet to the neareſt Part of the Powder; and, in theſe Caſes, we have ſeen, that the Theory agreed very well with the Experiments: But if a Bullet be placed at a greater Diſtance from

the Powder, ſuppoſe at 12, 18, or 24 Inches, we cannot then apply to the Motion of this Ball the ſame Principles which, in the 7th Propoſition, we have applied to ſuch as are contiguous to the Powder, or nearly ſo; for we have ſeen, in the laſt Propoſition, that, when the Surface of the fired Powder is not confined by a heavy Body, which it is obliged to impell before it, the Flame dilates itſelf with a Velocity much beyond what it can at any time communicate to a Bullet by its continued Preſſure; conſequently, as in the Diſtance of 12, 18, or 24 Inches, the Powder will have acquired a conſiderable Degree of this Velocity of Expanſion, the firſt Motion of the Ball will not be produced by the continued Preſſure of the Powder, but by the actual Percuſſion of the Flame; and it will therefore begin to move with a Quantity of Motion proportioned to the Quantity of this Flame, and the Velocities of its reſpective Parts.

From hence then it follows, that the Velocity of a Bullet, laid a conſiderable Diſtance before the Charge, ought to be greater, than what would be communicated to it by the Preſſure of the Powder acting in the Manner deſcribed in the 7th Propoſition; and this Deduction from our Theory we have confirmed by manifold Experience; by which we have found, that a Ball laid in the Barrel A, with its hinder Part 11 ½ Inches from its Breech, and impelled by 12 dw. of Powder, has acquired on its Diſcharge a Velocity of about 1400 Feet in 1″; when, if it had been acted on by the Preſſure of the Flame only, it would not have acquired a Velocity of 1200 Feet in 1″. The ſame we have found to hold true in all other greater Diſtances, (and alſo in leſſer, tho' not to the ſame Degree) and in all Quantities of Powder. And we have likewiſe found, that theſe Effects nearly correſpond with what was laid down in the laſt Propoſition,

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about the Velocity of Expanſion, and the elaſtic and unelaſtic Parts of the Flame.

And from hence too ariſes another Conſideration of great Conſequence in the Practice of Gunnery; which is, that no Bullet ſhould at any time be placed at any conſiderable Diſtance before the Charge, unleſs the Piece be extremely fortified; for a moderate Charge of Powder, when it has expanded itſelf through the vacant Space, and reaches the Ball, will, by the Velocity each Part has acquired, accumulate itſelf behind the Ball, and will thereby be condenſed prodigiouſly; whence, if the Barrel be not of an extraordinary Firmneſs in that Part, it muſt, by this reinforced Elaſticity of the Powder, infallibly burſt. The Truth of this Reaſoning I have experienced in an exceeding good *Tower*-Muſquet, forged of very tough Iron; for charging it with 12 dw. of Powder, and placing the Ball 16 Inches from the Breech, on the firing it, the Part of the Barrel juſt behind the Bullet was ſwelled out to double its Diameter, like a blown Bladder, and two large Pieces of two Inches long were burſt out of it.

Having ſeen that the entire Motion of a Bullet, laid at a conſiderable Diſtance from the Charge, is acquired by two different Methods, in which the Powder acts on it; the firſt being the Percuſſion of the Parts of the Flame, with the Velocity they had reſpectively acquired by expanding; the ſecond, the continued Preſſure of the Flame through the remaining Part of the Barrel; I endeavoured to ſeparate theſe different Actions, and to retain that only, which aroſe from the continued Preſſure of the Flame. For this Purpoſe, I no longer placed the Powder at the Breech, from whence it would have full Scope for its Expanſion, but I ſcattered it as uniformly as I could through the whole Cavity left behind the Bullet; imagining that, by this Means, the

the progreſſive Velocity of the Flame in each Part would be prevented by the Expanſion of the neighbouring Parts: And I found, that the Ball being laid 11 ½ Inches from the Breech, its Velocity, inſtead of 1400 Feet in 1″, which it acquired in the laſt Experiments, was now no more than 1100 Feet in 1″; which is 100 Feet ſhort of what, according to the Theory, ſhould ariſe from the continued Preſſure of the Powder only.

The Reaſon of this Deficiency was, doubtleſs, the inteſtine Motion of the Flame; for the Accenſion of the Powder, thus diſtributed through ſo much a larger Space than what it could fill, muſt have produced many Reverberations and Pulſations of the Flame; and from theſe internal Agitations of the Fluid, its Preſſure on the containing Surface will (as is the Caſe in all other Fluids) be conſiderably diminiſhed; and it has been in order to avoid this Irregularity, that in all the Experiments I have made, I have taken particular Care to have the Powder cloſely confined in as ſmall a Space as poſſible, even when the Bullet lay at ſome little Diſtance from it.

PROP. XIII.

To enumerate the various Kinds of Powder, and to deſcribe the propereſt Methods of examining its Goodneſs.

THE Powder, we have hitherto conſidered, is ſuppoſed to be ſuch, as is made for the Service of the Government; but, beſides this, there are many other Kinds, ſome better and ſome worſe, which I here propoſe to enumerate, as far as they have come to my Knowledge.

But, firſt, I muſt premiſe, that the Government Powder, if properly wrought, is, I believe, nearly as

as good, as any Powder made for general Uſe. I have examined it with great Care, and have compared it with other Powders made here in *England*, which are eſteemed the beſt, ſuch as the *Battle*, &c. and I cannot find any ſenſible Difference between them. I have likewiſe compared it, in frequent Trials, with ſome *Spaniſh* Powder, taken out of the *St. Jago* Prize; and tho', if I were to give my Opinion, I ſhould rather believe the *Spaniſh* Powder the better of the two, yet ſo ſmall an Inequality as a fiftieth or ſixtieth Part, which is the moſt that the Difference between them can amount to, is too little to be aſcertained with abſolute Certainty. I conceive too, by comparing the Experiments of others with my own, that the *French* Powder is little different from ours; altho' I cannot be ſo certain on this Head as I could wiſh, having never been able to procure any of their Powder myſelf. But it muſt be remember'd, that when I ſpeak of our Government Powder, it muſt be, what is ſuppoſed to be made of the Standard Proportion of Materials, and properly wrought; for ſuch was the Powder I made uſe of.

The ſtrongeſt Powder, I have yet met with, is ſome which, I am told, was made in *Holland*; its Force, compared with that of our Government Powder, is nearly as 5 to 4. But this Powder is undoubtedly made of the choiceſt pick'd Materials, and is probably wrought up with Spirits; ſo that Quantities of it could not be made, but at a much greater Expence, than what would be repaid by its additional Strength.

The next beſt Powder, that has come to my Hands, is a Powder made in *Portugal*, under the Direction of a *Dutchman*, who ſome Years ſince eſtabliſhed Powder-Mills near *Liſbon*. This is in Strength inferior to the *Dutch* Powder laſt mentioned;

tioned; but is, however, nearer to that than to our Government Powder.

The common Sale Powder here in *England*, ſuch as is to be had at every Grocer's, is much worſe than the Government or the *Battle* Powder, and extremely various, according to the Caprice of the Maker. I have tried ſome, whoſe Strength has been in Proportion to the Government Powder, as 2 to 3 nearly, and other Parcels have been ſtill worſe; but the worſt of all is the Powder made for the *African* Trade, uſually ſtyled *Guinea* Powder: But theſe weaker Powders are not worth Examination, as there is no eſtabliſhed Standard for their Compoſition.

Now theſe Differences in the Strength of Powder may ariſe from three Cauſes; either from the Quality of the Materials, from the Proportion obſerved in their Mixture, or from the Manner of working them together.

Powder, as is generally known, is compoſed of Saltpetre, Sulphur, and Charcoal: Of theſe Materials the Sulphur and Charcoal are much the cheapeſt; and tho' there are peculiar Kinds of theſe, which are fitteſt for this Purpoſe, yet the Expence of having the very beſt is ſo ſmall, when compared to the whole Coſt of the Powder, that it is ſtrange, if Powder, which would be otherwiſe good, is ſpoiled by bad Sulphur or Charcoal.

The moſt expenſive Part of the Compoſition, and conſequently the Part to which the Defects of Powder are oftner owing, is the Saltpetre. This is a Subſtance imbibed by the Earth from the Air; for a Quantity of Earth, which has had its Saltpetre waſhed out of it, will, when it has been expoſed to the Air for ſome time, produce Saltpetre again; and this as often as the Experiment ſhall be repeated.

Saltpetre

Saltpetre is of itſelf an uninflammable Subſtance; for if it be placed in the moſt violent Fire, it only melts, and never flames, provided no combuſtible Matter is permitted to mix with it: But tho' of itſelf, unmixed with other Bodies, it will neither flame nor burn; yet, if it be joined with burning Subſtances, it prodigiouſly augments the Violence of their burning; performing, in this Caſe, what the Air, forcibly mixed with Fire by the Blaſt of a Pair of Bellows, does in a much inferior Degree.

Powder then being a Mixture of Sulphur and Charcoal, which are very inflammable Subſtances, with Saltpetre, which in itſelf is not, if the Saltpetre be too much in Quantity, when compared with the other two, their burning may not be ſufficient to conſume the whole of the Saltpetre; whence the Fire may be leſs violent, and conſequently (according to what we have obſerved in the 10th Propoſition) the Powder leſs vigorous, than if ſome of the Saltpetre was taken away, and a like Quantity of the other Materials were added in its Stead. On the other hand, if the Saltpetre in the Compoſition be leſs, than what the burning of the other two Subſtances can eaſily conſume, the Fire will be leſs active than it ought to be, becauſe it is not augmented ſo much as it would be, if a larger Quantity of Saltpetre had been added to the Compoſition.

Hence then it appears, that the Goodneſs of Powder is not to be eſtimated only from the Quantity of Saltpetre contained in it, altho' that Subſtance ſeems to be the Baſis of the elaſtic Fluid, in which its Force conſiſts; for ſince the converting of the Saltpetre into that Fluid, and the Elaſticity of the Fluid afterwards, depend in ſome meaſure on the Violence of the Fire produced at the Exploſion, it is plain, that there is a certain Proportion in the Mixture of the Materials, which will beſt

contribute

contribute to this Purpoſe, and conſequently to the Perfection of the Powder.

What this Proportion is, has been aſcertained by Experience; and it ſeems now to be generally agreed, that in any Quantity of Powder ½ of it ſhould be Saltpetre, the remaining ½ conſiſting of equal Quantities of Sulphur and Charcoal. This is the Proportion followed by the *French*, and, I believe, by moſt Nations in *Europe*: We, indeed, pretend to a greater Degree of Nicety in our Proportions; tho', I am told, they do not greatly differ from what I have mentioned; nor am I convinced, that they are preferable: This I am ſure of, that no Methods of proving Powder, hitherto generally practiſed in *England*, could at all aſcertain the Difference; and other Powders, made with the uſual Proportions, are no whit inferior to ours.

But it is not the due Proportion of the Materials only, which is neceſſary to the making of good Powder; another Circumſtance, not leſs eſſential, is the mixing them well together; if this be not effectually done, ſome Parts of the Compoſition will have too much Saltpetre in them, and others too little; and in either Caſe there will be a Loſs of Strength in the Powder.

As the Excellency of Powder then depends on ſo many Particulars, in the Quality and Quantity of the Materials, and in the working them together, it is, doubtleſs, of great Importance, that thoſe who receive the publick Stores ſhould have it in their Power to ſatisfy themſelves about the Goodneſs of what is delivered to them. The Method moſt commonly followed of this Purpoſe, here with us, is (if I am rightly informed) to fire a ſmall Heap of it on a clear Board, and to attend nicely to the Flame and Smoke it produces, as likewiſe to the Marks it leaves behind it on the Table; from all which inſtructive Particulars the Merit of the Powder

der is aſcertained with great Accuracy, as is pretended: But beſides this uncertain Method, which, I preſume, (how much ſoever it may be practiſed) none will ſeriouſly undertake to defend, there are, on particular Occaſions, other Contrivances made uſe of; all which bear ſome Analogy to the common Powder-triers, ſold at the Shops; only they are more artfully fabricated, and inſtead of a Spring they move a Weight, which is a more certain and equable Power.

But theſe Machines, tho' more perfect than the common Powder-triers, are yet liable to great Irregularities; for as they are all moved by the inſtantaneous Stroke of the Flame, and not by its continued Preſſure, they do not determine the Force of the fired Powder with that Certainty and Uniformity, which were to be deſired in theſe kinds of Trials: And therefore, I cannot but think, the Method followed by the *French*, in the receiving of Powders by the Makers, to be much better. Their Practice is thus:

They have, in each Magazine, a ſmall Mortar caſt, with its Bed, according to a determined Pattern, which is the ſame throughout the Kingdom: This Mortar is always pointed at 45°, and it contains juſt three Ounces of Powder; and it is a ſtanding Maxim, that no Powder can be received into their Stores, unleſs three Ounces of it, placed in the Chamber of this Mortar, throws a ſolid Ball of 7 ½ Inches Diameter to the Diſtance of at leaſt 55 *French* Fathom.

It has been objected to this Method, that if each Barrel of Powder was to be proved in this manner, the Trouble of charging the Mortar, and bringing back the Ball each time, would be intolerable, and the Delay ſo great, that no Buſineſs of this kind could ever be finiſhed; and if a Number of Barrels are received on the Merit of a few, it is great

Odds

Odds but ſome bad ones will be amongſt them, which may prove a great Diſappointment in time of Service. Add to this another Exception, which to me has much more Weight; and that is, the monſtrous Diſproportion between the Weight of the Ball and the Powder that projects it; ſo that the Powder continues in Action a longer time, and expands through a much larger Space, in Proportion to its Quantities in theſe Trials, than it ever does in any real Service; whence it happens, that the Vapour cools, and great Part of it eſcapes thro' the Touch-hole, or by the Side of the Bullet; ſo that the Quantity of Motion produced by the Exploſion is, in this Inſtance, but little more than half of what it ought to be, if the Powder acted on the Ball with its full Force undiminiſhed by theſe Accidents; conſequently, as this Diminution of Force may not be always conſtant, the Action of the ſame Powder, by the varying of theſe adventitious Circumſtances, may, at different Times, convey the Ball to different Diſtances.

Now this laſt Exception does no ways hold againſt the Method by which I have tried the comparative Strength of different kinds of Powder, which has been by the actual Velocity given to a Bullet, by ſuch a Quantity of Powder as is uſually eſteemed a proper Charge for the Piece: And as this Velocity, however great, is eaſily diſcovered by the Motion which the Pendulum acquires from the Stroke of the Bullet, (according to the Principles laid down above) it might ſeem a good Amendment to the Method uſed by the *French*, to introduce this Trial by the Pendulum inſtead of it. But tho' I am ſatisfied, that this would be much more accurate, leſs laborious, and readier than the other, yet, as there is ſome little Attention and Caution required in this Practice, which might render it of leſs Diſpatch than might be convenient, when a great Number of

Barrels were to be ſeparately tried, I ſhould myſelf chooſe to practiſe another Method not leſs certain, but prodigiouſly more expeditious; ſo that I could engage, that the weighing out of a ſmall Parcel of Powder from each Barrel ſhould be the greateſt Part of the Labour; and, doubtleſs, three or four Hands could, by this Means, examine 500 Barrels in a Morning; beſides, the Machines for this Purpoſe, as they might be made of caſt Iron, would be ſo very cheap, that they might be multiplied at Pleaſure. However, I ſhall defer the Deſcription of this Method at preſent, and ſhall proceed to the Conſideration of the Reſiſtance of the Air, a Subject of the greateſt Importance to the Perfection of Gunnery.

F CHAP.

CHAP. II.

Of the Reſiſtance of the Air, and of the Track deſcribed by the Flight of Shot and Shells.

BEFORE I more minutely diſcuſs the Subject of this Chapter, it is neceſſary to premiſe, that the greateſt Part of Authors have eſtabliſhed it as a certain Rule, that, whilſt the ſame Body moves in the ſame Medium, it is always reſiſted in the duplicate Proportion of its Velocity; that is, if the reſiſted Body move in one Part of its Track with three times the Velocity with which it moved in ſome other Part, then its Reſiſtance to the greater Velocity will be nine times the Reſiſtance to the leſſer. If the Velocity in one Place be four times the Velocity in another, the Reſiſtance to the greater Velocity will be ſixteen times the Reſiſtance to the leſſer, and ſo on. This Rule, tho' exceſſively erroneous, (as we ſhall hereafter ſhew) when taken in a general Senſe, is yet undoubtedly very near the Truth, when confined within certain Limits; and therefore, in our future Diſquiſitions, we ſhall ſuppoſe, that in all ſmall Changes of Velocity in the reſiſting Body it does accurately hold true; ſo that when we ſpeak hereafter of the Reſiſtance of the Medium being increaſed or diminiſhed by the varying of the Velocity, we ſhall not hereby include that Increaſe or Diminution, which ought to take Place according to this Law, but ſhall thereby intend a Reſiſtance, greater

greater or leſs, than what the moving Body ought to undergo from the Application of this Principle; that is, we ſhall thereby underſtand an Increaſe or Diminution in the reſiſting Power of the Medium, ſimilar to what might be occaſioned by increaſing or diminiſhing its Denſity: the principal Purport of our preſent Attempt being to evince, that, according to the different Compreſſion of the Medium, or the different Degree of Velocity in the moving Body, ſuch Changes may ariſe in the reſiſting Power of the Medium, as could ſcarcely be effected, according to the Principles commonly received on this Subject, by a treble Augmentation of its Denſity. This we doubt not irrefragably to confirm in the following Diſſertation.

PROPOSITION I.

To deſcribe the general Principles of the Reſiſtance of Fluids to ſolid Bodies moving in them.

IN order to conceive the Reſiſtance of Fluids to a Body moving in them, it is neceſſary to diſtinguiſh between thoſe Fluids which, being compreſſed by ſome incumbent Weight, perpetually cloſe up the Space deſerted by the Body in Motion, without permitting for an Inſtant any Vacuity to remain behind it; and thoſe Fluids in which (they being not ſufficiently compreſſed) the Space left behind the moving Body remains for ſome time empty. Theſe Differences, in the reſiſting Fluids, will occaſion very remarkable Varieties in the Laws of their Reſiſtance, and are abſolutely neceſſary to be conſidered in the Determination of the Action of the Air on Shot and Shells; for the Air partakes of both theſe Affections, according to the different Velocities of the projected Body.

If a Fluid was ſo conſtituted, that all the Particles compoſing it were at ſome Diſtance from each other, and there was no Action between them, then the Reſiſtance of a Body moving therein would be eaſily computed, from the Quantity of Motion communicated to theſe Particles: For inſtance, if a Cylinder moved in ſuch a Fluid in the Direction of its Axis, it would communicate to the Particles it met with a Velocity equal to its own, and in its own Direction, ſuppoſing that neither the Cylinder, nor the Parts of the Fluid, were elaſtic; whence, if the Velocity and Diameter of the Cylinder be known, and alſo the Denſity of the Fluid, there would thence be determined the Quantity of Motion communicated to the Fluid, which (Action and Re-action being equal) is the ſame with the Quantity loſt by the Cylinder, conſequently the Reſiſtance would be hereby aſcertained.

In this kind of diſcontinued Fluid, the Particles being detached from each other, every one of them can purſue its own Motion in any Direction, at leaſt for ſome time, independent of the neighbouring ones; wherefore, if, inſtead of a Cylinder moving in the Direction of its Axis, a Body, with a Surface oblique to its Direction, be ſuppoſed to move in ſuch a Fluid, the Motion the Parts of the Fluid will hereby acquire, will not be in the Direction of the reſiſted Body, but perpendicular to its oblique Surface; whence the Reſiſtance to ſuch a Body will not be eſtimated from the whole Motion communicated to the Particles of the Fluid, but from that Part of it only, which is in the Direction of the reſiſted Body. In Fluids then, where the Parts are thus diſcontinued from each other, the different Obliquities of that Surface, which goes foremoſt, will occaſion conſiderable Changes in the Reſiſtance, altho' the Section of the ſolid by a plain Perpendicular to its Direction ſhould in all Caſes be the ſame. And Sir

Iſaac Newton has particularly determined, that in a Fluid thus conſtituted, the Reſiſtance of a Globe is but half the Reſiſtance of a Cylinder of the ſame Diameter, moving in the Direction of its Axis with the ſame Velocity.

But tho' the Hypotheſis of a Fluid, thus conſtituted, be of great Uſe in explaining the Nature of Reſiſtances; yet, in reality, no ſuch Fluid does exiſt within our Knowledge: All the Fluids with which we are converſant are ſo formed, that their Particles either lie contiguous to each other, or at leaſt act on each other in the ſame manner, as if they did; conſequently, in theſe Fluids, no one Particle, contiguous to the reſiſted Body, can be moved, without moving at the ſame time a great Number of others, ſome of which will be diſtant from it; and the Motion thus communicated to a Maſs of the Fluid will not be in any one determined Direction, but will in each Particle be different, according to the different Manner in which it lies in Contact with thoſe, from whence it receives its Impulſe; whence, great Numbers of the Particles being diverted into oblique Directions, the Reſiſtance of the moving Body, which will depend on the Quantity of Motion communicated to the Fluid in its own Direction, will be hereby different in Quantity, from what it would be in the preceding Suppoſition, and its Eſtimation becomes much more complicated and operoſe.

If the Fluid be compreſſed by the incumbent Weight of its upper Parts (as all Fluids are with us, except at their very Surface) and if the Velocity of the moving Body be much leſs than that with which the Parts of the Fluid would ruſh into a void Space, in conſequence of their Compreſſion, it is evident, that in this Caſe the Space left by the moving Body will be inſtantaneouſly filled up by the Fluid, and the Parts of the Fluid againſt which

the foremoſt Part of the Body preſſes in its Motion, will, inſtead of being impelled forwards in the Direction of the Body, circulate in ſome meaſure towards the hinder Part of the Body, thereby to reſtore the Equilibrium, which the conſtant Influx of the Fluid behind the Body would otherwiſe deſtroy; whence the progreſſive Motion of the Fluid, and conſequently the Reſiſtance of the Body, which depends thereon, would be in this Inſtance much leſs than in our firſt Hypotheſis, where each Particle was ſuppoſed to acquire, from the Stroke of the reſiſting Body, a Velocity equal to that, with which the Body moved, and in the ſame Direction. Sir *Iſaac Newton* has determined, that the Reſiſtance to a Cylinder moving in the Direction of its Axis, in ſuch a compreſſed Fluid as we have here treated of, is but one fourth Part of the Reſiſtance, which the ſame Cylinder would undergo, if it moved with the ſame Velocity in a Fluid conſtituted in the Manner, we have deſcribed in our firſt Hypotheſis, each Fluid being ſuppoſed to be of the ſame Denſity.

But again, it is not only in the Quantity of their Reſiſtance that theſe Fluids differ, but likewiſe in the different Manner, in which they act on Solids of different Forms moving in them.

We have ſhewn, that in the diſcontinued Fluid, which we firſt deſcribed, the Obliquity of the foremoſt Surface of the moving Body would diminiſh the Reſiſtance; but in compreſſed Fluids this holds not true, at leaſt not in any conſiderable Degree; for the principal Reſiſtance in compreſſed Fluids ariſes from the greater or leſſer Facility, with which the Fluid, impelled by the Fore-part of the Body, can circulate towards its hindermoſt Part; and this being little, if at all, affected by the Form of the moving Body, whether it be cylindrical, conical, or ſpherical, it follows, that while the tranſverſe Sec-

tion

tion of the Body, and conſequently the Quantity of impelled Fluid be the ſame, the Change of its Figure will ſcarcely affect the Quantity of its Reſiſtance.

And this Caſe, that is, the Reſiſtance of a compreſſed Fluid to a ſolid, moving in it with a Velocity much leſs than what the Parts of the Fluid would acquire from their Compreſſion; this Caſe, I ſay, has been very fully conſidered by Sir *Iſaac Newton*, who has aſcertained the Quantity of ſuch a Reſiſtance according to the different Magnitudes of the moving Body, and the Denſity of the Fluid. But he very expreſsly informs us, that the Rules he has laid down are not generally true, but upon a Suppoſition that the Compreſſion of the Fluid be increaſed in the greater Velocities of the moving Body: However, ſome unſkilful Writers who have followed him, overlooking this Caution, have applied his Determinations to Bodies moving with all kinds of Velocities, without attending to the different Compreſſions of the Fluids they were reſiſted by; and by this Means they have accounted the Reſiſtance of the Air to Muſket and Cannon-ſhot to be but one third Part of what I have found it by Experience.

Indeed, from all we have ſaid, it appears plain enough, that the reſiſting Power of the Medium muſt be increaſed, when the reſiſting Body moves ſo faſt, that the Fluid cannot inſtantaneouſly preſs in behind it, and fill the deſerted Space; for when this happens, the Body will be deprived of the Preſſure of the Fluid behind it, which in ſome meaſure ballanced its Reſiſtance, and muſt ſupport on its Fore-part the whole Weight of a Column of the Fluid, independant of the Motion it gives to the Parts of the Fluid; and beſides, the Motion in the Particles driven before the Body is, in this Caſe, leſs affected by the Compreſſion of the Fluid, and conſequently they are leſs deflected from the Di-

rection in which they are impelled by the reſiſted Surface; whence this Species of Reſiſtance approaches more and more to that deſcribed in our firſt Hypotheſis, where each Particle of the Fluid being unconnected with the neighbouring ones, it purſued its own Motion, in its own Direction, without being interrupted or deflected by their Contiguity; and therefore, as we before obſerved, that the Reſiſtance of a diſcontinued Fluid to a Cylinder, moving in the Direction of its Axis, was four times greater than the Reſiſtance of a Fluid ſufficiently compreſſed of the ſame Denſity, it follows, that the Reſiſtance of a Fluid, when a Vacuity is left behind the moving Body, may be near four times greater than that of the ſame Fluid, when no ſuch Vacuity is formed; for when a void Space is thus left, we have ſhewn the Reſiſtance to approach in its Nature to that of a diſcontinued Fluid.

This then may probably be the Caſe in a Cylinder moving in the ſame compreſſed Fluid, according to the different Degrees of its Velocity; ſo that if it ſet out with a great Velocity, and moves in the Fluid till that Velocity be much diminiſhed, the reſiſting Power of the Medium may be near four times greater in the Beginning of its Motion than in the End. In a Globe the Difference will not be ſo great, becauſe, on account of its oblique Surface, its Reſiſtance in a diſcontinued Medium is but about twice as much as in one properly compreſſed; for its oblique Surface diminiſhes its Reſiſtance in one Caſe and not in the other: however, as the Compreſſion of the Medium, even when a Vacuity is left behind the moving Body, may yet confine the oblique Motion of the Parts of the Fluid, which are driven before the Body, and as in an elaſtic Fluid (as is our Air) there will be ſome Degree of Condenſation in thoſe Parts, it is highly probable, that the Reſiſtance of a Globe, moving in a com-

preſſed

preſſed Fluid with a very great Velocity, will be between that of a Globe and of a Cylinder, in a diſcontinued Medium; that is, (in Proportion to its Velocity) we may ſuppoſe it to be more than twice, and leſs than four times the Reſiſtance of the ſame Globe, moving ſlowly through the ſame Medium; whence, perhaps, we ſhall not much err in ſuppoſing the Globe in its ſwifteſt Motions to be reſiſted near three times more, in Proportion to its Velocity, than when it is ſloweſt.

And as this Increaſe of the reſiſting Power of the Medium will take Place, when the Velocity of the moving Body is ſo great, that a perfect Vacuity is left behind it, ſo ſome Degree of Augmentation will be ſenſible in Velocities much ſhort of this; for even when, by the Compreſſion of the Fluid, the Space left behind the Body is inſtantaneouſly filled up, yet if the Velocity, with which the Parts of the Fluid ruſh in behind, is not much greater than that, with which the Body moves, the ſame Reaſons we have urged above, in the Caſe of an abſolute Vacuity, will hold in a leſs Degree in this Inſtance; and therefore we are not to ſuppoſe, that the increaſed Reſiſtance, which we have hitherto treated of, immediately vaniſhes, when the Compreſſion of the Fluid is juſt ſufficient to prevent a *Vacuum* behind the reſiſted Body; but we muſt conſider it as diminiſhing only, according as the Velocity with which the Parts of the Fluid follow the Body, exceeds that with which the Body moves.

Hence then we conclude, that if a Globe ſets out in a reſiſting Medium, with a Velocity much exceeding that with which the Particles of the Medium would ruſh into a void Space, in conſequence of their Compreſſion, ſo that a *Vacuum* is neceſſarily left behind the Globe in its Motion, the Reſiſtance of this Medium to the Globe will be near three times greater, in Proportion to its Velocity, than what we are ſure, from Sir *Iſaac Newton*, would

would take Place in a ſlower Motion. We may farther conclude, that the reſiſting Power of the Medium will gradually diminiſh, as the Velocity of the Globe decreaſes, till at laſt, when it moves with Velocities, which bear but a ſmall Proportion to that, with which the Particles of the Medium follow it, the Reſiſtance becomes the ſame with what is aſſigned by Sir *Iſaac Newton* in the Caſe of a compreſſed Fluid.

And from this Determination we may learn, how falſe that Poſition is, which aſſerts the Reſiſtance of any Medium to be in the duplicate Proportion of the Velocity of the reſiſted Body; for it plainly appears, by what we have ſaid, that this can only be conſidered as nearly true in ſmall Variations of Velocity, and can never be applied in the comparing together the Reſiſtances to all Velocities whatever without the moſt enormous Errors.

Theſe Principles being laid down, we ſhall next proceed to an experimental Examination of the Reſiſtance of the Air in particular, both in order thence to evince how nearly theſe Speculations agree to the real obſerved Action of Fluids, and likewiſe to ſhew, how egregiouſly all thoſe Theoriſts have been miſtaken, who have conceived, that the Reſiſtance of the Air to Shells and Shot of all kinds was ſcarcely worthy of Attention.

PROP. II.

To determine the Reſiſtance of the Air to Projectiles by Experiments.

BY Means of the Machine deſcribed in the 8th Propoſition, I have it in my Power to determine the Velocity, with which a Ball moves in any Part of its Track, provided I can direct the Piece ſo as to cauſe the Bullet to impinge on the Pendulum

lum placed in that Part; and therefore charging a Muſket-barrel three times ſucceſſively with a leaden Ball of $\frac{3}{4}$ of an Inch Diameter, and about half its Weight of Powder, and taking ſuch Precaution in the weighing of the Powder, and placing it, that I was aſſured, by many previous Trials, that the Velocity of the Ball could not differ by 20 Feet in 1″ from its Medium Quantity, I fired it againſt the Pendulum placed at 25 Feet, at 75 Feet, and at 125 Feet Diſtance from the Mouth of the Piece reſpectively; and I found that it impinged againſt the Pendulum in the firſt Caſe with a Velocity of 1670 Feet in 1″, in the ſecond Caſe with a Velocity of 1550 Feet in 1″, and in the third Caſe with a Velocity of 1425 Feet in 1″; ſo that in paſſing thro' 50 Feet of Air, the Bullet loſt a Velocity of about 120 or 125 Feet in 1″; and the Time of its paſſing thro' that Space being about $\frac{1}{11}$ or $\frac{1}{10}$ of 1″, the Medium Quantity of Reſiſtance muſt, in theſe Inſtances, have been about 120 times the Weight of the Ball, which (as the Ball was nearly $\frac{1}{12}$ of a Pound) amounts to about 10 lb. Avoirdupoiſe.

Now if a Computation be made according to the Method laid down for compreſſed Fluids in the 38th Propoſition, Lib. 2. of Sir *Iſaac Newton*'s *Principia*, ſuppoſing the Weight of Water to be to the Weight of Air, as 850 to 1, it will be found, that the Reſiſtance to a Globe of $\frac{3}{4}$ of an Inch Diameter, moving with a Velocity of about 1600 Feet in 1″, will not, on thoſe Principles, amount to any more than a Force of $4\frac{1}{6}$ lb. Avoirdupoiſe; whence, as we know, that the Rules contained in that Propoſition are very accurate in ſlow Motions, we may hence conclude, that the reſiſting Power of the Air in ſlow Motions is leſs than in ſwift Motions in the Ratio of $4\frac{1}{6}$ to 10, a Proportion between that of 1 to 2 and 1 to 3.

Again,

Again, I charged the ſame Piece, a Number of times, with equal Quantities of Powder, and Balls of the ſame Weight, taking all poſſible Care to give to every Shot an equal Velocity; and firing three times againſt the Pendulum, placed 25 Feet only diſtant from the Mouth of the Piece, the Medium of the Velocities with which the Ball impinged, was nearly that of 1690 Feet in 1″: Then removing the Piece 175 Feet from the Pendulum, I found, taking the Medium of five Shots, that the Velocity, with which the Ball impinged at this Diſtance, was that of 1300 Feet in 1″; whence the Ball, in paſſing through 150 Feet of Air, loſt a Velocity of about 390 Feet in 1″; and the Reſiſtance computed from theſe Numbers comes out ſomething more than in the preceding Inſtance, it amounting here to between 11 and 12 Pounds, Avoirdupoiſe; whence, according to theſe Experiments, the reſiſting Power of the Air to ſwift Motions is greater than in ſlow ones in a Ratio which approaches nearer to the Ratio of 3 to 1, than in the preceding Experiments.

Having thus aſcertained the Reſiſtance to a Velocity of near 1700 Feet in 1″, which muſt be allowed to be more than ſufficient for leaving a *Vacuum* behind the Ball, I next examined the Reſiſtance to ſmaller Velocities; and for this Purpoſe I charged the ſame Barrel with Balls of the ſame Diameter, but with leſs Powder; and placing the Pendulum at 25 Feet Diſtance from the Piece, I fired againſt it five times with an equal Charge each time; the Medium Velocity, with which the Ball impinged, was that of 1180 Feet in 1″; then removing the Pendulum to the Diſtance of 250 Feet, the Medium Velocity of five Shots made at this Diſtance was that of 950 Feet in 1″; whence the Ball, in paſſing through 225 Feet of Air, loſt a Velocity of 230 Feet in 1″; and as it paſſed through

through that Interval in about $\frac{3}{4}$ of 1″, the Reſiſtance to the Middle Velocity will come out to be near 33 $\frac{1}{3}$ times the Gravity of the Ball, or 2 lb. 10 oz. Avoirdupoiſe. Now the Reſiſtance to the ſame Velocity, according to the Laws obſerved in ſlower Motions, amounts to $\frac{7}{11}$ of the ſame Quantity; whence, in a Velocity of 1065 Feet in 1″, the reſiſting Power of the Air is augmented in no greater a Proportion than that of 7 to 11; whereas we have ſeen in the former Experiments, that to ſtill greater Degrees of Velocity, the Augmentation approached very near to the Ratio of 1 to 3.

But farther, I fired three Shot, of the ſame Size and Weight with thoſe already mentioned, over a large Piece of Water; ſo that their dropping into the Water being very diſcernable, both the Diſtance and Time of their Flight might be accurately aſcertained; each Shot was diſcharged with a Velocity of 400 Feet in 1″; and I had ſatisfied myſelf, by many previous Trials of the ſame Charge with the Pendulum, that I could rely on this Velocity to 10 Feet in 1″. The firſt Shot flew 313 Yards in 4″ $\frac{1}{4}$, the ſecond flew 319 Yards in 4″, and the third 373 Yards in 5″ $\frac{1}{2}$. According to the Theory of Reſiſtance eſtabliſhed for ſlow Motions, the firſt Shot ought to have ſpent no more than 3″, 2, in its Flight, the ſecond 3″, 28, and the third 4″; whence it is evident, that every Shot was retarded conſiderably more than it ought to have been, had that Theory taken Place in its Motion; conſequently, the reſiſting Power of the Air is very ſenſibly increaſed, even in ſo ſmall a Velocity as that of 400 Feet in 1″.

From all that we have related then, it appears, that the Theory of the Reſiſtance of the Air, eſtabliſhed in ſlow Motions by Sir *Iſaac Newton*, and confirmed by many Experiments, is altogether erroneous, when applied to the ſwifter Motions of Muſket

Muſket or Cannon-ſhot; for that, in theſe Caſes, the reſiſting Power of the Medium is augmented to near three times the Quantity aſſigned by that Theory; that, however, this increaſed Power of Reſiſtance diminiſhes as the Velocity of the reſiſted Body diminiſhes, till at length, when the Motion is ſufficiently abated, the actual Reſiſtance coincides with that ſuppoſed in the Theory; that therefore the Reſiſtance is not in the duplicate Proportion of the Velocity of the moving Body, as is uſually aſſerted, but varies from that Proportion according to the different Compreſſion of the Fluid compared with the Velocity; conſequently, from the Conſideration of theſe Particulars, we may venture to aſſert, that whilſt the Reſiſtance of the Air was thus imperfectly and faultily conceived, the Track of Muſket and Cannon-ſhot through that Medium could not be aſcertained with the leaſt Degree of Certainty; and therefore the Art of Gunnery could not but continue extremely imperfect: However, it is not ſufficient to have ſhewn the Reſiſtance to be augmented in great Velocities, beyond what has been uſually ſuppoſed; for, that we may be enabled more definitely to compute the Motion of Projectiles, it is neceſſary that we ſhould aſſign the Rate of this Augmentation according to the different Velocities of the reſiſted Body. This ſhall be the Subject of our next Propoſition.

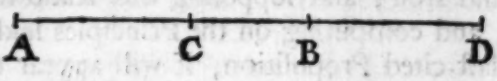
PROP. III.

To aſſign the different Augmentations of the reſiſting Power of the Air according to the different Velocities of the reſiſted Body.

AS no large Shot are ever projected in Practice with Velocities exceeding that of 1700 Feet in 1″, I have not as yet made any Experiments on the

the Reſiſtance of Bodies which have moved with a ſwifter Motion than this, eſteeming the Determination of the Variation of the Reſiſtance to all leſſer Velocities, to be ſufficient for the Purpoſes of this Treatiſe.

According to the Trials I have made, the reſiſting Power of the Air to Velocities leſs than that of 1700 Feet in 1″, may be thus nearly exhibited.



Let AB be taken to AC in the Ratio of the Velocity of 1700 Feet in 1″, to the given Velocity to which the reſiſting Power of the Air is required; continue the Line AB to D, ſo that BD may be to AD, as the reſiſting Power of the Air to ſlow Motions is to its reſiſting Power to a Velocity of 1700 Feet in 1″, then ſhall CD be to AD, as the reſiſting Power of the Air to ſlow Motions is to its reſiſting Power to the given Velocity repreſented by AC.

PROP. IV.

To determine the Velocities with which Muſket and Cannon-ſhot are diſcharged from their reſpective Pieces by their uſual Allotment of Powder.

FROM the Computations of the 7th Propoſition of the 1ſt Chapter, confirmed by the ſucceeding Experiments, it plainly appears, that a leaden Ball of $\frac{4}{5}$ of an Inch in Diameter, and weighing nearly 1 $\frac{1}{2}$ oz. Avoirdupoiſe, if it be fired from a Barrel of 45 Inches in Length, with half its Weight of Powder, will iſſue from that Piece with

with a Velocity which, if it were uniformly continued, would carry it near 1700 Feet in 1″.

If inſtead of a leaden Ball an Iron one of the ſame Diameter was placed in the ſame Situation in the ſame Piece, and was impelled by the ſame Quantity of Powder, the Velocity of ſuch an Iron Bullet would be greater than that of the leaden one, in the ſubduplicate Ratio of the ſpecific Gravities of Lead and Iron; and ſuppoſing that Ratio to be as 3 to 2, and computing on the Principles laid down in the laſt-cited Propoſition, it will appear that an Iron Bullet of 24 lb. Weight, ſhot from a Piece of 10 Feet in Length, with 16 lb. of Powder, will acquire from the Exploſion a Velocity which, if uniformly continued, would carry it nearly 1650 Feet in 1″.

This is the Velocity which, according to our Theory, a Cannon-ball of 24 lb. Weight is diſcharged with, when it is impelled by a full Charge of Powder; but if, inſtead of a Quantity of Powder weighing two-thirds of the Ball, we ſuppoſe the Charge to be only half the Weight of the Ball, then its Velocity will, on the ſame Principles, be no more than at the Rate of 1490 Feet in 1″; and the ſame would be the Velocities of every leſſer Bullet, fired with the ſame Proportions of Powder, if the Lengths of all Pieces were conſtantly in the ſame Ratio with the Diameters of their Bore: And altho', according to the uſual Dimenſions of the ſmaller Pieces of Artillery, this Proportion does not always hold, yet the Difference is not conſiderable enough to occaſion a very great Variation from the Velocities here aſſigned; as will be obvious to any one, who ſhall make a Computation thereon.

But in theſe Determinations, we ſuppoſe the Windage to be no more, than is juſt neceſſary for the eaſy putting down the Bullet; whereas, in real Service, either through Negligence or Unſkifulneſs,

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it often happens, that the Diameter of the Bore ſo much exceeds the Diameter of the Bullet, that great Part of the inflamed Fluid eſcapes by its Side; whence the Velocity of the Shot, in this Caſe, may be conſiderably leſs than what we have aſſigned. However, Part of this may poſſibly be compenſated by the greater Heat, which (as we have obſerved in the 6th Propoſition) in all Probability attends the firing of theſe large Quantities of Powder.

COROLLARY.

From the great Velocity of Cannon-ſhot aſſigned in this Propoſition we may clear up that Difficulty which has driven ſome Writers, on the common Theory of Gunnery, into a very extraordinary Hypotheſis. The Difficulty, I mean, is the Extent of the ſuppoſed point-blank Shot, or the Diſtance to which it is conceived to fly in a ſtraight Line. Our *Anderſon* having found, by many Experiments, that the Track of Shells and Bullets, in the firſt Part of their Motion, was much leſs incurvated, than what it ought to be on the Principles of *Galileo*, when compared with the diſtant Ranges, he ſuppoſed, in order to reconcile this Circumſtance with his Theory, that every Shot was impelled to a certain Diſtance from the Mouth of the Piece, in a ſtraight Line, or that for ſome Diſtance it was no ways affected by the Action of Gravity. By this Means he defended, as he thought, the Hypotheſis of a parabolic Motion, and at the ſame time aſſented to the vulgar Opinion of the practical Writers, who, in general, aſſerted the ſame thing. But could no better Account be given of his Experiments, it would yet be unneceſſary, I preſume, formally to confute ſo ſtrange a Suppoſition as that of the Suſpenſion of the Action of Gravity. Indeed, *Anderſon* was deceived, by his not knowing how

greatly the primitive Velocity of the heavieſt Shot is diminiſhed in the Courſe of its Flight by the Reſiſtance of the Air. And the received Opinion of practical Gunners is not more difficult to account for, ſince, when they agree, that every Shot flies in a ſtraight Line to a certain Diſtance from the Piece, which imaginary Diſtance they have denominated the Extent of the point-blank Shot, we need only ſuppoſe, that within that Diſtance, which they thus determine, the Deviation of the Path of the Shot from a ſtraight Line is not very perceptible in their Method of pointing. Now, as a Shot of 24 lb. fired with two-thirds of its Weight in Powder, will, at the Diſtance of 500 Yards from the Piece, be ſeparated from the Line of its original Direction, by an Angle of little more than half a Degree; thoſe, who are acquainted with the inaccurate Methods often uſed in the directing of Cannon, will eaſily allow, that ſo ſmall an Aberration as this may, by the generality of Practitioners, be unattended to, and the Path of the Shot may conſequently be deemed a ſtraight Line, eſpecially as other Cauſes of Error will often intervene, much greater than what ariſes from the Incurvation of this Line by Gravity.

In the preſent Propoſition, the Velocity of a Shot is determined, both when fired with two-thirds of its Weight of Powder, and with half its Weight of Powder, reſpectively; and, on this Occaſion, I muſt remark, that on the Principles of the Theory, which we have aſcertained in this Treatiſe, the increaſing the Charge of Powder will increaſe the Velocity of the Shot, till the Powder arrives at a certain Quantity; after which, if the Powder be increaſed, the Velocity of the Shot will diminiſh. The Quantity producing the greateſt Velocity, and the Proportion between that greateſt Velocity and

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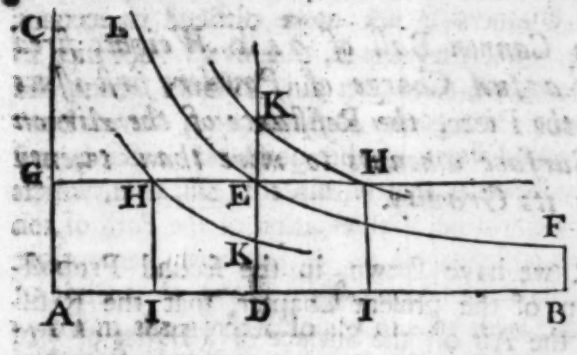
the Velocity communicated by greater and leſſer Charges, may be thus aſſigned:



Let A B repreſent the Axis of the Piece; draw A C perpendicular to it, and to the Aſymptotes A C and A B, deſcribe any Hyperbola L F, and draw B F Parallel to A C; find out now the Point D, where the Rectangle A D E G is equal to the Hyperbolic Area DEFB, then will A D repreſent that Heighth of the Charge, which communicates the greateſt Velocity to the Shot; whence A D being to A B, as 1 to 2,71828, as appears by the Table of Logarithms, from the Length of the Line A D thus determined, and the Diameter of the Bore, the Quantity of Powder contained in this Charge is eaſily known.

If, inſtead of this Charge, any other filling the Cylinder to the Heighth A I be uſed, draw I H Parallel to A C, and through the Point H, to the ſame Aſymptotes A C and A B, deſcribe the Hyperbola H K; then the greateſt Velocity will be to the Velocity communicated by this Charge A I, in the ſubduplicate Proportion of the Rectangle A E, to the ſame Rectangle diminiſhed by the trilinear Space H K E. All this eaſily follows from the Principles laid down in the 7th Propoſition of the 1ſt Chapter.

PROP. V.

When a Cannon-Ball of 24 lb. *Weight, fired with a full Charge of Powder, firſt iſſues from the Piece, the Reſiſtance of the Air on its Surface amounts to more than twenty times its Gravity.*

FOR we have ſhewn, in the ſecond Propoſition of the preſent Chapter, that the Reſiſtance of the Air on the Surface of a Bullet of ¼ of an Inch Diameter, moving with a Velocity of 1670 Feet in 1″, amounted to 10 lb. Now we have ſeen, in the laſt Propoſition, that an Iron Bullet weighing 2¼ lb. if fired with 16 lb. of Powder, (which is uſually eſteemed its proper battering Charge) acquires a Velocity of about 1650 Feet in 1″, ſcarcely differing from the other; whence, as the Surface of this laſt Bullet is more than fifty-four times greater than the Surface of a Bullet of ¼ of an Inch in Diameter, and their Velocities are nearly the ſame, it follows, that the Reſiſtance on the largerBullet will amount to more than 540 lb. which is near twenty-three times its own Weight.

SCHOLIUM.

We have obſerved, in the Introduction, that the Theoriſts who have profeſſedly written on the Subject of Gunnery, have generally agreed in ſuppoſing the Flight of Shot and Shells to be nearly in the Curve of a Parabola; and it is againſt this Hypotheſis that the two laſt Propoſitions are particularly aimed.

For the Reaſon, which has been given by theſe Authors, in Suppoı of their Opinion, is the ſuppoſed

poſed inconſiderable Reſiſtance of the Air; ſince, as it is agreed on all Sides, that the Track of Projectiles would be a Parabola, if there was no Reſiſtance, it has from hence been too raſhly concluded, that the Interruption, which the ponderous Bodies of Shells and Bullets would receive from ſo rare a Medium as the Air, would be ſcarcely ſenſible, and conſequently that their parabolic Flight would be hereby ſcarcely affected.

Now the prodigious Reſiſtance of the Air to a Bullet of 24 lb. Weight, ſuch as we have here eſtabliſhed it, ſufficiently confutes this Reaſoning; for how erroneous muſt that Hypotheſis be, which neglects as inconſiderable a Force, which amounts to more than twenty times the Gravity of the moving Body? However, we ſhall not content ourſelves with having demonſtrated the Reality and Quantity of the Air's Reſiſtance, but we ſhall proceed to a more particular Examination of the Flight of Bodies in that Medium, where we ſhall evince, by many Experiments, how greatly the Track, deſcribed by almoſt every Projectile, deviates in every Circumſtance from what it ought to be on the generally-received Principles. But, firſt, it is neceſſary to aſſume a few Particulars, the Demonſtrations of which may be found in almoſt every Writer on the common Theory of falling Bodies.

Poſt. 1. If the Reſiſtance of the Air be ſo ſmall, that the Motion of a projected Body be in the Curve of a Parabola, then the Axis of that Parabola will be perpendicular to the Horizon, and conſequently the Part of the Curve, in which the Body aſcends, will be equal and ſimilar to that in which it deſcends.

Poſt. 2. If the Parabola, in which the Body moves, be terminated on a horizontal Plain, then the Vertex of the Parabola will be equally diſtant from its two Extremities.

Poſt. 3. Alſo the moving Body will fall on that horizontal Plain in the ſame Angle and the ſame Velocity with which it was firſt projected.

Poſt. 4. If a Body be projected in different Angles, but with the ſame Velocity, then its greateſt horizontal Range will be, when it is projected in an Angle of 45° with the Horizon.

Poſt. 5. If the Velocity, with which the Body is projected, be known, then this greateſt horizontal Range may be thus found: Compute, according to the common Theory of Gravity, what Space the projected Body ought to fall through to acquire the Velocity with which it is projected, then twice that Space will be the greateſt horizontal Range, or the horizontal Range, when the Body is projected in an Angle of 45° with the Horizon.

Poſt. 6. The horizontal Ranges of a Body, when projected with the ſame Velocity, at different Angles, will be between themſelves, as the Sines of twice the Angle, in which the Line of Projection is inclined to the Horizon.

Poſt. 7. If a Body be projected in the ſame Angle with the Horizon, but with different Velocities, the horizontal Ranges will be in the duplicate Proportion of thoſe Velocities.

Theſe Poſtulates contain the Principles on which the Motions of Projectiles are computed by the modern Writers on the Art of Gunnery.

If any of theſe Poſtulates hold not true, when applied to the Motion of a Projectile, then that Projectile deviates in its Flight from a parabolic Track; we ſhall therefore effectually deſtroy the common Theory of Projectiles, if we can ſhew, that in general none of theſe Poſtulates correſpond to the obſerved Motions of thoſe Bodies.

PROP.

PROP. VI.

The Track deſcribed by the Flight of Shot or Shells is neither a Parabola, nor nearly a Parabola, unleſs they are projected with ſmall Velocities.

FOR we have determined, in the fourth Propoſition of the preſent Chapter, that a Muſket-ball $\frac{1}{4}$ of an Inch in Diameter, fired with half its Weight of Powder from a Piece 45 Inches long, moves with a Velocity of near 1700 Feet in 1″. Now, if this Ball flew in the Curve of a Parabola, its horizontal Range at 45° would be found, by the fifth Poſtulate, to be about 17 Miles. Now all the practical Writers aſſure us, that this Range is really ſhort of half a Mile. *Diego Uſano* aſſigns to an Arquebuſe, 4 Feet in Length, and carrying a leaden Ball of $1\frac{1}{2}$ oz. Weight (which is very near our Dimenſions) an horizontal Range of 797 common Paces, when it is elevated between 40 and 50 Degrees, and charged with a Quantity of fine Powder equal to the Weight of the Ball. *Merſennus* too tells us, that he found the horizontal Range of an Arquebuſe at 45° to be leſs than 400 Fathom, or 800 Yards; whence, as either of theſe Ranges are ſhort of half an *Engliſh* Mile, it follows, that a Muſket-ſhot, when fired with a reaſonable Charge of Powder, at an Elevation of 45°, flies not the $\frac{1}{14}$ Part of the Diſtance it ought to do, if it moved in a Parabola.

Nor is this great Contraction of the horizontal Range to be wondered at, when it is conſidered, that the Reſiſtance of this Bullet, when it firſt iſſues from the Piece, amounts to 120 times its Gravity, as has been experimentally demonſtrated in the ſecond Propoſition of the preſent Chapter.

Again, leſt it ſhould be ſaid, that this Aberration of the Flight of a Muſket-ball from the Curve of a Parabola is no Proof, but that heavier Shot, whoſe Reſiſtance is much leſs in Proportion to their Weight, may ſufficiently coincide with the common Hypotheſis; our next Inſtance ſhall be in an Iron Bullet of 24 lb. Weight, which is the heavieſt in common Uſe for Land Service. Such a Bullet, fired from a Piece of the cuſtomary Dimenſions, with its greateſt Allotment of Powder, has a Velocity of 1650 Feet in 1″, as we have determined in the fourth Propoſition of the preſent Chapter. Now if the horizontal Range of this Shot, at 45°, be computed on the parabolic Hypotheſis by the fifth Poſtulate, it will come out to be about 16 Miles, which is between five and ſix times its real Quantity; for the practical Writers all agree in making it leſs than three Miles: And St. *Remy* informs us of ſome Experiments made by Mr. *Du Metz*, in which the Range, at 45°, of a Piece ten Feet in Length, carrying a Ball of 24 lb. and charged with 16 lb. of Powder, was 2250 *French* Fathom, which is 222 Fathom ſhort of three Miles; conſequently an Iron Bullet of 24 lb. Weight, when impelled with its full Allotment of Powder, flies not, at 45°, to the fifth Part of the Diſtance which it ought to do, if it deſcribed the Curve of a Parabola.

But farther, it is not only when Projectiles are moved with theſe very great Velocities, that their Flight ſenſibly varies from the Curve of a Parabola; the ſame Aberration often takes place in ſuch as move ſlow enough to have their Motion traced out by the Eye; for there are few Projectiles, that can be thus examined, which do not viſibly diſagree with the firſt, ſecond, and third Poſtulate, they obviouſly deſcending through a Curve which is ſhorter and leſs inclined to the Horizon than that

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in which they aſcended; alſo the higheſt Point of their Flight, or the Vertex of the Curve, is much nearer to the Place, where they fall on the Ground, than to that from whence they were at firſt diſcharged. Theſe Things cannot be a Moment doubted of by one, who in a proper Situation views the Flight of Stones, Arrows or Shells thrown to any conſiderable Diſtance.

I have found too by Experience, that the fifth, ſixth and ſeventh Poſtulates are exceſſively erroneous, when applied to the Motions of Bullets moving with ſmall Velocities: A leaden Bullet $\frac{1}{4}$ of an Inch in Diameter, diſcharged with a Velocity of about 400 Feet in 1″, and in an Angle of 19°: 5′, with the Horizon, ranged on the horizontal Plane, no more than 448 Yards; whereas its greateſt horizontal Range being found by the fifth Poſtulate to be at leaſt 1700 Yards, the Range at 19°: 5′ ought, by the ſixth Poſtulate, to have been 1050 Yards; whence, in this Experiment, the Range was not $\frac{1}{7}$ of what it muſt have been, had the common-received Theory been true.

Again, a Ball was fired with the ſame Velocity as in the laſt Experiment, but at an Elevation of 9°: 45′, its horizontal Range was at a Medium 330 Yards.

Now this Range, according to the fifth and ſixth Poſtulates, (if its original Velocity be conſidered) ſhould have been 566 Yards. But if it were to be deduced from the laſt Experiment, by Means of the ſixth Poſtulate, it ſhould have been no more than 241 Yards; either of which Numbers are extremely diſtant from 330.

Again, a Ball being fired at an Elevation of 8°, but with a Velocity of 700 Feet in 1″, the horizontal Range at a Medium was 690 Yards.

But computing this Range from the original Velocity of the projected Body, according to the fifth

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and ſixth Poſtulates, we ſhall find, that if the Theory, on which thoſe Poſtulates are founded, could be relied on, the Range in the preſent Inſtance ought to have been 2400 Yards; whence it appears, that the Body flew not to half the Diſtance which, had it moved in a Parabola, it ought to have done.

Again, a Ball being fired with the ſame Velocity as in the laſt, but at an Elevation of 4°, its horizontal Range was 600 Yards.

Now this Range, if deduced from the laſt Experiment by the ſixth Poſtulate, ſhould not have been more than 350 Yards; hence then is evinced the Falſity of that Poſtulate, and conſequently of the parabolic Hypotheſis, on which it is founded.

Having thus proved, that the Track deſcribed by the Flight, even of the heavieſt Shot, is neither a Parabola, nor approaching to a Parabola, except when they are projected with very ſmall Velocities; we ſhall refer to a ſecond Part, a more diſtinct Explication of the Nature of the Curve which theſe Bodies really trace out in their Motion through the Air: But, as a Specimen of the great Complication of that Subject, I ſhall here inſert an Account of a very extraordinary Circumſtance, which frequently takes Place therein.

As Gravity acts perpendicularly to the Horizon, it is evident, that if no other Power but Gravity deflected a projected Body from its rectilinear Courſe, its Motion would be conſtantly performed in a Plane perpendicular to the Horizon, paſſing through the Line of its original Direction: But we have found, that the Body in its Motion often deviates from this Plane, ſometimes to the Right-hand, and at other times to the Left; and this in an incurvated Line, which is convex towards that Plane; ſo that the Motion of a Bullet is frequently in a Line, having a double Curvature, it being bent towards the Horizon by the Force of Gravity,

and

and again bent out of its original Direction to the Right or Left by the Action of ſome other Force: In this Caſe no Part of the Motion of the Bullet is performed in the ſame Plane, but its Track will lie in the Surface of a kind of Cylinder, whoſe Axis is perpendicular to the Horizon. The Truth of this Aſſertion we ſhall evince by indiſputable Experiments.

PROP. VII.

Bullets in their Flight are not only depreſſed beneath their original Direction by the Action of Gravity, but are alſo frequently driven to the Right or Left of that Direction by the Action of ſome other Force.

IF it was true, that Bullets varied their Direction by the Action of Gravity only, then it ought to happen, that the Errors, in their Flight to the Right or Left of the Mark they were aimed at, ſhould increaſe in the Proportion of the Diſtance of the Mark from the Piece only: But this is contrary to all Experience; the ſame Piece, which will carry its Bullet within an Inch of the intended Mark, at 10 Yards Diſtance, cannot be relied on to 10 Inches in 100 Yards, much leſs to 30 Inches in 300 Yards. This Increaſe of the Uncertainty of the Shot in great Diſtances, more than in the Proportion of thoſe Diſtances, muſt have been obſerved by all who have been at any time converſant with the practical Part of Artillery. Now this Inequality can only ariſe from the Track of the Bullet being incurvated ſideways as well as downwards; for by this Means the Diſtance between that incurvated Line, and the Line of Direction, will increaſe in a much greater Ratio than that of the Diſtance; theſe Lines being coincident at the Mouth

Mouth of the Piece, and afterwards ſeparating in the Manner of a Curve and its Tangent, if the Mouth of the Piece be conſidered as the Point of Contact.

But that thoſe, who have not been themſelves accuſtomed to theſe Matters, may entertain no Doubt about what we here aſſert, I ſhall recite ſome Experiments, I have made, which will put the Matter out of all Queſtion.

I took a Barrel carrying a Ball of ¼ of an Inch Diameter, and fixing it on a heavy Carriage, I ſatisfied myſelf of the Steadineſs and Truth of its Direction, by firing at a Board 1 ½ Foot ſquare, which was placed at 180 Feet Diſtance; for I found, that in 16 ſucceſſive Shot I miſſed the Board but once. Now the ſame Barrel being fixed on the ſame Carriage, and fired with a ſmaller Quantity of Powder, ſo that the Shock on the Diſcharge would be much leſs, and conſequently the Direction leſs changed, I found, that at 760 Yards Diſtance, the Ball flew ſometimes 100 Yards to the Right of the Line it was pointed on, and at other times 100 Yards to the Left. I found too, that its Direction in the perpendicular Line was not leſs uncertain, it falling one time above 200 Yards ſhort of what it did at another; altho', by the niceſt Examination of the Piece after the Diſcharge, it appeared not to have the leaſt ſtarted from the Poſition it was placed in.

This then ſufficiently confirms the Propoſition, ſince it was impoſſible the Bullet could have flown in the Manner here deſcribed, had not the Line of its Flight been bent round to the Right or Left as well as downwards.

SCHOLIUM.

The Reality of this doubly-incurvated Track being thus demonſtrated, it may perhaps be aſked,

What

What can be the Cauſe of a Motion ſo different from what has been hitherto ſuppoſed? And to this I anſwer, that the Deflection in Queſtion muſt be owing to ſome Power acting obliquely to the progreſſive Motion of the Body, which Power can be no other than the Reſiſtance of the Air. If it be farther aſked, How the Action of the Reſiſtance of the Air can at any time be in a Line oblique to the progreſſive Motion of the Body? I farther reply, that it may ſometimes ariſe perhaps from Inequalities in the reſiſted Surface, but that its general Cauſe is doubtleſs a whirling Motion acquired by the Bullet about its Axis; for by this Motion of Rotation, combined with the progreſſive Motion, each Part of the Bullet's Surface will ſtrike the Air in a Direction very different from what it would do if there was no ſuch Whirl; and the Obliquity of the Action of the Air ariſing from this Cauſe will be greater, according as the rotary Motion of the Bullet is greater in Proportion to its progreſſive Motion.

I have now finiſhed all that I propoſed to determine in this Place, relating to the Force of Powder and the Reſiſtance of the Air: But as the Knowledge of the Reſiſtance of Solids to the Penetration of Shot is of great Importance in the practical Part of Gunnery, eſpecially in battering in Breach, I ſhall end the preſent Treatiſe with a Propoſition relating thereto, which is as follows.

PROP.

PROP. VIII.

If Bullets of the ſame Diameter and Denſity impinge on the ſame ſolid Subſtance with different Velocities, they will penetrate that Subſtance to different Depths, which will be in the duplicate Ratio of thoſe Velocities nearly And the Reſiſtance of ſolid Subſtances to the Penetration of Bullets is uniform.

THE firſt Part of this Propoſition I have found to be true in a great Number of Inſtances; for when a leaden Bullet, $\frac{1}{4}$ of an Inch in Diameter, was fired againſt a ſolid Block of Elm, with a Velocity of about 1700 Feet in 1″, I found that in a great Number of Trials it had penetrated from $4\frac{1}{2}$ to $5\frac{1}{2}$ Inches deep. When a Bullet of the ſame Size was fired againſt the ſame Block, with a Velocity of about 730 Feet in 1″, its outer Surface was always near $\frac{1}{4}$ of an Inch within the Surface of the Wood, ſo that its Penetration was at a Medium about 1 Inch; or, if the Cavity be conſidered, and reduced to a Cylinder, about $\frac{7}{8}$ of an Inch, and with a Velocity of 400 Feet in 1″, the Bullet penetrated the ſame Block uſually to about half its Subſtance, which, reduced to a cylindric Cavity, is $\frac{1}{4}$ of an Inch in Depth.

Now 55, 10, 3, are nearly in the duplicate Proportion of theſe Velocities; whence, if the Penetration to the greateſt Velocity be ſuppoſed 5 Inches, the Penetration of the others ought by the Propoſition to be $\frac{10}{11}$ and $\frac{1}{11}$ of an Inch reſpectively; and theſe Numbers ſcarcely differ from $\frac{7}{8}$ and $\frac{1}{4}$, which are what we have found in our Experiments; a greater Coincidence than this cannot be expected, when the unequal Texture of the ſame Piece of Wood, and the

the Change of the Form of the Bullet by the Stroke, are conſidered.

Now, from the Penetration being in the duplicate Proportion of the Velocity of the impinging Body, the uniform Reſiſtance of the Wood is eaſily evinced on the ſame Principles, that the uniform Action of Gravity is demonſtrated, from its communicating to falling Bodies Velocities in the duplicate Proportion of the Spaces they deſcend thro', or from the riſing of Bodies when projected upwards, to Heights which are in the duplicate Proportion of the Velocities with which they begin to aſcend.

FINIS.